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Presentata da: Dott. Roberto Angeloni

Coordinatore Dottorato Prof. Roberto Giuntini

Relatore Prof. Antonio Cadeddu

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Contents

To Simona and Edoardo Carlo

FOREWORD

The present dissertation is the attempt to give a thematic account of Niels Bohr's scientific and philosophical thought. Starting from epistemological considerations as regards Imre Lakatos' Methodology of Scientific Research Programs, it was my aim to go back to the origin of the heuristic underlying Bohr's research program. Therefore, I came to integrate Lakatos' view on methodology with Gerald Holton's idea of thematic origin of scientific thought. Following these premises, I came to a thesis, perhaps surprising, that Bohr's basic methodological principle was the condition of continuity rather than discontinuity. Moreover, almost as a consequence of the thematic analysis, it became a priority of this work to establish connections between so-called external factors and the "rational" development of the program. However, the dissertation's aim could not be confined to understand how the social, cultural and historical background may shape scientific discoveries. Therefore I refused to restrict the thesis to the quarrel on the importance's degree of the cultural relationships between Niels Bohr and his mentor and professor of philosophy, Harald Høffding. On the contrary, step by step, by focusing on the study of the historical origin of the hypothesized themata (unity, totality, harmony, continuity) to a certain extent, I became aware of what Mara Beller termed the fundamental unity or harmony between the possibilities of definition and observation, this is to say the place where the above mentioned heuristic becomes physical. As it will be shown later on, continuity is the condition of such a unity – or, as it is more appropriate to call it, asymptotic unity. In this view, the indispensability of the concepts of classical physics finds its role with respect to quantum reality in a new perspective, i.e. the continuous interaction between those two terms. It is here that the concept of complementarity unfolds its more profound meaning. And it is here that such a view appears to match with a Romantic interpretation of the rational generalization thesis, where the quantum of action, with its atomicity, empirical unity, indivisibility, is elevated to an *a priori* concept that must be re-comprehended.

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A special thank is given to Dr. Finn Aaserud, Niels Bohr Archive's Director and editor of the *Collected Works* of Niels Bohr, who, with his constant help and valuable advices, especially during the period of my stay in Copenhagen, at the Niels Bohr Institute, gave me the opportunity to mature a new perspective of the history of quantum physics. Moreover, I would like to thank all the friends of the Niels Bohr Archive, who delighted my stay throughout the months in Copenhagen, in particular to Mrs Felicity Pors, who helped me in the translation into English of some unpublished manuscripts from Bohr's private correspondence.

A very special thank is given to Prof. Jan Faye, from the University of Copenhagen, who enlightened me with his precious epistemological suggestions.

This work is dedicated to my wife, Simona, incomparable and indispensable part of my life, who gave me the joy of our first son, Edoardo Carlo.

Last but not least, I would like to thank my parents for their constant encouragement in doing my work.

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Chapter 1

Introduction

1.1 An Outlook on the Cultural Background of Non-classical Interpretation of Physics in the Early Twentieth Century

Since the early 1960s, history of quantum physics' main topics were brought to the forefront of the debate of the history of science. Even in this subject we meet with a controversy between an internalistic and an externalistic view. The internalistic approach focuses on the study of the development of a discipline by analysing the relations among theories within the discipline itself. For example, with regard to the history of quantum physics, one should explain Niels Bohr's first atomic theory of 1913 in terms of what can follow from theories and data in the physics of the previous times. The externalistic view regards scientific theories as not only influenced by the previous "technical history" of the discipline, but also by sociological and cultural circumstances beyond the domain of the discipline itself. Considering the example of Niels Bohr's first atomic theory, we should supplement the merely scientific account with Bohr's cultural and philosophical background.

It means that such an explanation should be related also to the study of social, cultural, and political context. Sometimes the externalists forget to deal with the purely scientific accounts and emphasize only the sociological and psychological factors in the development of science. Some others, the internalists fail to admit the existence of certain methodological decisions, which render scientific research so unique an enterprise. They are based on presuppositions not backed by unquestionable results, as they arise from un-verifiable and un-falsifiable statements.

Nevertheless, such a debate is no more an up-to-date one, as it presupposes a well identifiable line of demarcation between internal and external factors that according to some philosopher of science would correspond to the boundary between rational and irrational historical reconstruction respectively. It is my aim to propose a new mode of considering the relations between internal and external factors in the history of science through the modification of Imre Lakatos' methodology of scientific research programs for explaining Niels Bohr's rational generalization thesis.

In my opinion, only a profound revision of the approach to the history of science might allow the historian of science to shed light on such a riddle within Bohr's scientific program. So far neither adopting the internal nor the external approach was sufficient to give account of how such continuity thesis may coexist with Niels Bohr's cultural background that historians of science have regarded as influenced by the concept of discontinuity.

One of the least-discussed and least-understood parts of Bohr's interpretation is his thesis that quantum theory is a rational generalization of classical mechanics. Bohr himself wrote:

«The endeavours to formulate general laws for these possibilities and probabilities by suitably limited application of the concepts of the classical theories have led recently, after a series of phases in its development, to the creation of a rational quantum mechanics by means of which we are able to describe a very wide range of experience, and which may be regarded in every respect as a generalization of the classical physical theories»[1].

This thesis is necessary for understanding Bohr's approach to quantum physics: his view on complementarity, the correspondence principle, and the indispensability of classical concepts. Although Bohr considered Planck's quantum of action as leading to the need for a revision in physics, he was more a continuity theorist than a revolutionary. Bohr was a continuity theorist in the sense that he tried to maintain and emphasize those features of the predecessor theory that were preserved in the transition to the successor theory. It was his fundamental belief in this continuity that he was trying to call attention to by describing quantum theory as a rational generalization of classical mechanics[2]. It is my aim to explain the origin of Bohr's fundamental belief in continuity and harmony through an historical analysis of his cultural and scientific background in the light of a post Lakatosian methodology of scientific research program. As we shall see, such a methodological revision will match with Gerald Holton's thematic approach.

Historians and philosophers of science of the kind of Max Jammer and Gerald Holton carried on remarkable studies and reflections on the influence of the philosophical climate on scientific works, in particular on the philosophical background of quantum mechanics. Jammer pointed out that physicists usually refuse of declaring themselves as subscribing to a particular school of philosophical thought, even if they are aware of

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[1] NIELS BOHR, "Introductory Survey", in *Atomic Theory and the Description of Nature* (Cambridge: Cambridge University Press, [1929], 1934), 1-24, p. 4. Reprinted in JØRGEN KALCKAR (ed.), *Niels Bohr Collected Works* [NBCW], Vol. 6: Foundations of Quantum Physics I, 1926-1932 (Amsterdam: North-Holland, 1985), 279-302, p. 282.

[2] Cf. PETER BOKULICH, ALISA BOKULICH, "Niels Bohr's Generalization of Classical Mechanics", *Foundations of Physics*, 2005, 35: 347-369, p. 349.

belonging to it[3]. Gerald Holton subscribed this point and affirmed that scientists, consciously or not, use thematic presuppositions in the nascent "private" period of their works. He noted that behind the process of theory formation stands a set of individual presuppositions, about which the scientist may not be very conscious because they are part of the "private" science[4]. Jammer added that philosophical considerations influence the physicist's mind and they act "more like an undercurrent beneath the surface than like a patent well-defined guiding line"[5]. Both Holton and Jammer agree that it is the nature of science to suppress these motivating preconceptions, but it is the historian of science's duty to recover them under the superstructure of the scientific edifice. Correspondence, autobiographical remarks minutes and letters can supply information that is not detectable in scientific publications.

Namely, Jammer sustained that the documentary material may show that certain philosophical ideas of the late nineteenth century helped the formation of the new conceptions of the modern quantum theory. He dwelled on the importance of writers such as Renouvier, Boutroux, Kierkegaard, Møller, Peirce, James and Høffding.

In particular, Jammer held that Søren Kierkegaard inspired Niels Bohr. According to Jammer, Harald Høffding, the other great Danish philosopher and Bohr's professor of philosophy at the university, mediated the influence. Bohr attended Høffding's lectures at the University of Copenhagen and read Høffding's works in which Kierkegaard's philosophy occupied a prominent place. Since Høffding visited William James in 1904 in the United States and later on lectured on his psychology, Jammer took it for granted

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3 Cf. MAX JAMMER, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966), p. 166.

4 Cf. GERALD J. HOLTON, *Victory and Vexation in Science: Einstein, Bohr, Heisenberg and others* (Cambridge, MA: Harvard University Press, 2005), p. 140.

5 JAMMER, *The Conceptual Development* (cit. note 3), p. 166.

that James too inspired Bohr. For the sake of clearness, we have to remind ourselves that Jammer proposed an analogy between Kierkegaard's "leap" when speaking of attitudes to life and Bohr's "leap" when speaking of a transition from one stationary state to another. An interesting reference to James can be found in Klaus M. Meyer-Abich, *Korrespondenz, Individualität und Komplementarität*, from 1965. He considered James' influence on Bohr as possible without drawing any conclusions.

In his article "The Roots of Complementarity" from 1970, Gerald Holton emphasized Kierkegaard and James' influence on Bohr. Also in Silvano Tagliagambe, "Il concetto di realtà fisica e il principio di complementarità", 1972, and in Evandro Agazzi, *Temi e problemi di filosofia della fisica*, 1974, we find a partial acceptance of Jammer's ideas. Just like Lewis S. Feuer did in his *Einstein and the Generations of Science*, 1974.

Enrico Bellone and Nadia Robotti gave an interesting account on the relevance of the Bohr-Høffding relationship on: "Alcuni aspetti dello sviluppo dei primi modelli sulla costituzione dell'atomo", *Contributi alla storia della Meccanica Quantistica*, 1976. The authors emphasized the importance of Bohr's philosophical background – in particular, some philosophical issues of Høffding's thought, namely the qualitative dialectic – for explaining Bohr's method of searching for coherence in the new ideas after he had stressed the conflict between the classical theory and his model.

Also Klaus Stolzenburg contributed to the debate with his, *Die Entwicklung des Bohrschen Komplementaritätsgedankens in den Jahren 1924 bis 1929*, from 1977. Furthermore Kurt Hübner saw an analogy between Kierkegaard and Bohr in his *Critique of Scientific Reason*, 1983.

According to John Hendry's *The Creation of Quantum Mechanics and the Bohr-Pauli Dialogue*, 1984, there are striking parallels between Bohr's atomic model and some aspects of Kierkegaard's philosophy.

Dugald Murdoch in his *Niels Bohr's Philosophy of Physics*, 1987, sustained that Høffding influenced Bohr's pragmatism. Sandro Petruccioli in *Atoms, Metaphors, and Paradoxes: Niels Bohr and the Construction of a New Physics*, from 1988, focused on the role played by the atomic model in constructing the theory. Professor Petruccioli's study re-evaluated the role of metaphor in science by regarding them as an irreplaceable component of the linguistic mechanism of scientific theories. In particular, "electrons" and "elliptic orbits" served to make explicit the content of a metaphorical expression that Bohr applied to the new quantum theoretical concept of stationary state, which had enabled the theory to explain the spectroscopic laws and a radioactive behaviour of atoms.

Petruccioli suggested that this initial metaphor carried out a precise heuristic function in originating a research program that made it possible to utilize every aspect of the classical theories in the systematic construction of quantum theory[6].

Jan Faye in *Niels Bohr: His Heritage and Legacy*, 1991, maintained that there was a close connection between Niels Bohr's approach to the study of the atom and the philosophical influences, which shaped his outlook from childhood and youth onwards. On the contrary, David Favrholdt's thesis in *Niels Bohr's Philosophical Background*, 1992, is that Bohr's works in physics should be explained in terms of an internalistic rather than an externalistic view on the history of science. Favrholdt was the scholar

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[6] Cf. SANDRO PETRUCCIOLI, *Atomi, Metafore, Paradossi. Niels Bohr e la costruzione di una nuova fisica* (Roma: Theoria, 1988), English translation by Ian McGilvray: *Atoms, Metaphors, and Paradoxes: Niels Bohr and the Construction of a New Physics* (Cambridge University Press, 1993), p. 91.

who inaugurated the "Danish debate" on Niels Bohr's philosophical background with the article "Niels Bohr and Danish Philosophy" issued on the *Danish Yearbook of Philosophy* in 1976. In 1979 Jan Faye published on the same review his "The Influence of Harald Høffding's Philosophy on Niels Bohr's Interpretation of Quantum Mechanics". Favrholdt's promptly reply was issued on the same number of the *Danish Yearbook*.

Since his first article Favrholdt sought to refute the theses about the inspiration Bohr presumably received from Danish philosophy. On the contrary, Faye's aim was to demonstrate the close connection that existed between Niels Bohr's attitude to the atomic world and the philosophical influence he was under already from childhood and youth.

Paraphrasing Léon Rosenfeld, there cannot be much doubt that Niels Bohr like everyone was influenced by his own cultural and social context. That does not mean that Bohr was not an independent thinker. One way may be dispensed with: that Bohr developed the first atomic theory because he was a genius, and, since geniuses do not belong to mundane history like most people, it is not noteworthy to ask about the cultural or philosophical conditioning of his theory.

There is general consensus among historians of science that talk of genius does not so much explain scientific innovation as re-describe it.

Therefore, it would be misleading trying to demonstrate whether either Bohr developed his epistemological ideas single-handed or, if not, how he was able to do it with no-more philosophical preparation than Høffding's elementary course of lectures[7]. It

[7] Cf. LÉON ROSENFELD, "Book Review. Max Jammer, The Conceptual Development of Quantum Mechanics", *Nuclear Physics*, 1969, A 126: 696.

should be rather a priority to ascertain in which way the cultural background may inspire the development of a scientific discipline.

Jammer and Holton held of something central when they stress the importance of those motivating preconceptions underlying scientific research, but, as I see it, they did not show how these presuppositions interact with proper scientific reflection to such an extent that they could exert heuristic functions within the research program.

Now it is worth taking a brief detour from the most relevant studies carried on in the history of science for illustrating what is reputed to be the philosophical background of non-classical interpretation of quantum theory. Moreover, I shall give account of the insufficiencies of most of the historical proposals so far carried on. As we have seen, Max Jammer was one of the contributors to the historical tradition better known as externalism.

Jammer remarked that the interpretation of optical phenomena required the use of "mutually exclusive notions", which were foreign to the traditional conceptions. A critical examination and epistemological analysis of the foundations of the theory was hence considered necessary.

He commenced by presenting Renouvier's rejection of actual infinity that was for him "a logical self-contradiction and an empirical falsehood"[8]. Renouvier rejected continuity because continuity presupposes an actual infinity of gradations. Renouvier refused the concept of causality both as an idealistic category of the understanding in the Kantian sense, and as a realistic principle of order in the cosmos.

As Jammer specified:

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[8] CHARLES RENOUVIER, *Les Principes de la Nature* (Paris: Colin, 1864), p. 37: «Je dis que si toute la suite des nombres entiers était actuellement donnée il y aurait deux nombres égaux dont l'un serait plus grand que l'autre, ce qui est une contradiction formelle, *in terminis* […]».

«Renouvier proposed a phenomenalism according to which all that we immediately know is but a particular phenomenon or "representation". Every representation has a two-fold character: it is a "representing" and a "represented" ("representatif" and "représenté"); it is an experience of something and it is something experienced»[9].

Later on, Jammer attributed similar ideas to Boutroux: in particular, a philosophy of nature based on contingency:

«Analysing the notion of natural law, as seen in the sciences themselves, I found that this law is not a first principle but rather a result; that life, feeling, and liberty are true and profound realities, whereas the relatively invariable and general forms apprehended by science are but the inadequate manifestations of these realities. […]. All experimental finding is reduced, in the end, to confining within as close limits as possible the value of the measurable phenomena. We never reach the exact points at which the phenomenon really begins and ends. Moreover, we cannot affirm that such points exist, except, perhaps, in indivisible instants; a hypothesis which, in all probability, is contrary to the nature of time itself. Thus we see, as it were, only the containers of things, not the things themselves. We do not know if things occupy, in their containers, an assignable place. Supposing that phenomena were indeterminate, though only in a certain measure insuperably transcending the range of our rough methods of reckoning, appearances would none the less be exactly as we see them. Thus, we attribute to things a purely hypothetical if not unintelligible determination when we interpret literally the principle by which any particular phenomenon is connected with any other particular phenomenon»[10].

Charles Sanders Peirce formulated the theory of tychism (chance), according to which "chance is a factor in the universe"[11] and hence concluded that deterministic mechanics

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[9] JAMMER, *The Conceptual Development* (cit. note 3), p. 167.

[10] EMILE BOUTROUX, *De la Contingence des Lois de la Nature* (Paris: Baillière, 1874), translated into English by F. ROTHWELL, *The Contingency of the Laws of Nature* (Chicago: Open Court Publishing, 1920), p. 28. Cf. also O. BOELITZ, *Die Lehre vom Zufall bei Emile Boutroux* (Leipzig: Quelle und Meyer, 1907).

[11] DOROTHY C. HARTSHORNE, PAUL WEISS (eds.), *Collected Papers of Charles Sanders Peirce*, VI (Cambridge, MA: Harvard University Press, 1935), p. 137.

with its necessarily reversible laws cannot account for the undeniable existence of growth and evolution in nature[12].

The deterministic philosophy, according to Peirce, is alone unable to explain reality. Furthermore, Peirce regarded determinism as incapable to prove its position empirically. The essence of its position was "that certain continuous quantities have certain exact values […]" but observation cannot determine the value of such a quantity "with a probable error absolutely *nihil*"[13]. Through the analysis of the process of experimental observation he concluded that absolute chance is an irreducible factor in physical process.

«Try to verify any law of nature, and you will find that the more precise your observations, the more certain they will be to show regular departure from the law. We are accustomed to ascribe these, and I do not say wrongly to errors of observation; yet we cannot usually account for such errors in any antecedently probable way. Trace their causes back far enough and you will be forced to admit they are always due to arbitrary determination, or chance»[14].

Another original interpretation before the advent of modern quantum mechanics was that of the Viennese physicist Exner, who applied a statistical interpretation to macroscopic bodies, which apparently showed a deterministic behaviour. In 1919, in one of his "Lectures on the Physical Foundations of Science", Franz S. Exner asserted:

«From a multitude of events […] laws can be inferred which are valid for the average state of this multitude (macrocosm) whereas the individual event (microcosm) may remain undermined. In this sense the principle of causality holds for all macroscopic occurrences without be necessarily valid for the microcosm. It also follows that the laws of the macrocosm are not absolute laws but rather laws of

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[12] JAMMER, *The Conceptual Development* (cit. note 3), p. 168.

[13] HARTSHORNE, WEISS (eds.), *Collected Papers* (cit. note 11), p. 46.

[14] *Ibidem*.

probability; whether they hold always and everywhere remains to be questioned […]. Every single, however, specialized, physical measurement produces only an average value resulting from billions of individual motions […] but to predict in physics the outcome of an individual process is impossible»[15].

Professor Jammer regarded the philosophical schools of contingentism, existentialism, pragmatism, and logical empiricism as risen in reaction to traditional rationalism. Therefore, he saw their affirmation as the rejection of an abstract intellectualism that culminated in the doctrine of free will and the denial of mechanical determinism or the principle of causality. As a consequence – Jammer affirmed – these "currents of thoughts" contributed to build the philosophical background for modern quantum mechanics. Jammer also had entertained the idea that the above thinkers might have influenced Bohr. But Bohr never mentioned them, albeit it is very likely that he had heard of them as representatives of the modern philosophy of discontinuity. At the most Bohr might have regarded these ideas as "indicative of the winds of doctrine of the time", to borrow Henry Folse's expression, and hence they prepared his mind for what was to come.

However, most part of the scholars usually mentions four main figures as supposed sources of inspiration for Bohr's theories: the Danish Philosopher and theologist Søren Kierkegaard (1813-1855), the American philosopher and psychologist William James (1842-1910), the Danish writer Poul Martin Møller (1794-1838) and the Danish philosopher Harald Høffding (1843-1931). Nevertheless, there are conflicting views on what is the crucial element in Bohr's background. I will seek to discuss and illustrate

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[15] FRANZ S. EXNER, *Vorlesungen über die Physikalischen Grundlangen der Naturwissenschaften* (Vienna: Deuticke, 1919; 2nd ed. 1922), pp. 705-6.

the following hypotheses one by one in order to propose a possible solution to such an historical puzzle.

1.2 The Kierkegaard-Hypothesis

Max Jammer stressed the importance of both the historical origin and philosophical background of certain epistemological ideas, which culminated in the so-called "Copenhagen interpretation" of quantum physics. Jammer sustained that the Danish precursor of modern existentialism and neo-orthodox theology, Søren Kierkegaard, through his influence on Bohr, affected also the course of modern physics. The historical proofs – according to Jammer – are "certain references and allusions in Bohr's philosophical writings"[16]. Moreover, he also regarded Harald Høffding as a pupil and a follower of Kierkegaard's thought.

With regard to the so-called "allusions in Bohr's writing", unfortunately, Jammer "forgot" to mention them, therefore they do not stand for a proof of Bohr's philosophical orientation towards Kierkegaard. Harald Høffding was influenced by Kierkegaard although he didn't fully embraced Kierkegaard's thought. In fact, Høffding was critical of his theory of the stages, i.e. the idea that the possible ways of living are mutually exclusive. Høffding was a philosopher of continuity and he shared with Kierkegaard the rejection of "a system of life" in the Hegelian sense. It is worth noticing that Kierkegaard criticized Hegel of building up a philosophical system that did not leave any room for individual existence. However, not as a follower but as a scholar, Høffding wrote *Kierkegaard som filosof*, published in 1892. This book was the best monography of Kierkegaard's thought at the time. Whenever in his youth Niels

[16] JAMMER, *The Conceptual Development* (cit. note 3), p. 172.

Bohr had become interested in Kierkegaard, he could probably have started reading Høffding's book. Nonetheless, Bohr had many other occasions to deepen his philosophical interests such as there were other ways in which Høffding could exert his cultural influence on his students, as we shall see later on.

Whatever role Høffding might have played in spreading Kierkegaard's philosophy, in my opinion, Jammer seems to make an unjustifiable use of analogies between the ideas, which characterized the cultural climate in which Bohr lived, and the concepts at the basis of quantum physics in a way that might suggest an undue direct influence of the cultural background on scientific entrepreneurship.

As a matter of fact, Jammer held that Kierkegaard's philosophy of life and religion, his so-called "qualitative dialectic", his antithesis between thought and reality, his contradictory conception of life, and his insistence of the necessity of choice had apparently left a deep impression on Bohr's youthful mind. Even if such a cultural debt had been historically well documented, it would be a surreptitious conclusion to hold that Bohr transposed Kierkegaard's philosophy into his scientific program. The relations, if any, must be found on a methodological level.

That does not prevent that Bohr was really interested in Kierkegaard. Niels sent his brother Harald as a birthday gift Kierkegaard's book, *Stadier paa Livets Vej* (Stages on Life's Way), written in 1845, with a letter dated April 20, 1909:

«I am sending you herewith (in addition to what Mother is so sweet to send you in my name) Kierkegaard's "Stages on Life's Road". That is the only thing I have to send; nevertheless, I don't think I could easily find anything better. In any case, I have enjoyed reading it very much, in fact, I think it is something of the finest I have ever read. Now I am looking forward to hear your opinion of it»[17].

In another letter to Harald, April 26, Niels expressed his disagreement with Kierkegaard:

«When you some day have read the “Stages”, what you by no means must hurry with, you shall hear a little from me: for, I have written a few remarks about it (not in agreement with K.); but I do not intend to be so trite with my poor nonsense as to spoil the impression of so beautiful a book»[18].

We can imagine that Niels Bohr could have enjoyed the aesthetic experience and the moral passion, without having to agree also with the antiscientific attitude of much of the work.

Kierkegaard’s emphasis on the role of discontinuity between incompatibles, on the “leap” rather than the gradual transition, on the inclusion of the individual, was as non-classical in philosophy as the elements of the “Copenhagen doctrine” – quantum jumps, probabilistic causality, observer-dependent description, duality – were to be in physics. As Harald Høffding wrote:

«Kierkegaard’s leading idea was that the different possible conceptions of life are so sharply opposed to one another that we must make a choice between them, hence his catchword, *either-or*; moreover, it must be a choice which each particular person must make for himself, hence his second catchword *the individual*. He himself designated his thought “qualitative dialectic”, by which he meant to bring out its opposition to the doctrine taught by Romantic speculation of continuous development by means of

[17] ROSENFELD, JENS RUD NIELSEN (eds.), *Niels Bohr Collected Works*, Vol. 1: Early Work, 1905-1911 (Amsterdam, NL: North Holland Publishing Company, 1972), p. 501.

[18] *Ibidem*, p. 503.

necessary inner transitions. Kierkegaard regarded this doctrine as pure fantasticalness, a fantasticalness, to be sure, to which he himself had felt attracted»[19].

This is to say that Kierkegaard's "qualitative dialectic" was an acceptance – according to Gerald Holton's interpretation – of thesis and antithesis, without proceeding to another stage at which the tension is resolved in a synthesis. Holton, as Jammer did, hypothesized that the so-called Kierkegaard's "leaps" might have inspired Bohr in the sense of providing a sort of sympathetic preparation[20]. But, in my opinion, it would be misleading to pretend to demonstrate that Bohr made a direct transposition of Kierkegaard's ideas into a physical context.

As Professor David Favrholdt pointed out[21], discontinuity became a reality to physicists when Max Planck discovered the quantum of action in 1900. Einstein's theory about the photoelectric effect was another important fact to be considered. Further investigations into radioactivity demonstrated that causal laws ought to be replaced by statistical descriptions.

Niels Bohr wrote about this issue in a paper to his professor of physics Christian Christiansen:

«The reason for speaking of an average life-span [for atoms of a radioactive element – DF] without indicating a fixed starting point is that the atoms so to speak do not age before they suddenly go to pieces, so that the chance [probability] of them going to pieces is the same at any point in their lifetime. This is

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[19] HARALD HØFFDING, *Den nyere Filosofis Historie* (Copenhagen: Det Nordisk Forlag, 1894), English transl. by B.E. MEYER (ed.) *A History of Modern Philosophy* , Vol. II (New York: Dover, 1955), p. 286.

[20] Cf. HOLTON, "On the Roots of Complementarity", *Dædalus*, 1970, 99: 1015-55. Reprinted in HOLTON, *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge, MA: Harvard University Press, 1973), pp. 130-33.

[21] DAVID FAVRHOLDT, *Niels Bohr's Philosophical Background* (Copenhagen: Munksgaard, 1992), p. 56.

what is expressed by the potential law of decrease and by the equation derived from this to the effect that the number of atoms that go to pieces is dependent alone on the number of atoms present»[22].

We infer that Bohr renounced causal explanations and held spontaneous changes before he got acquainted with Kierkegaard's existentialism.

Professor Silvano Tagliagambe, one of the adherents of Jammer's views, saw the following quotation from Bohr's lecture, "The Unity of Human Knowledge" (1960), as an example of Kierkegaard's description of the attitudes to life, which mutually exclude each other[23]:

«Confronted with the great diversity of cultural developments, we may therefore search for those features in all civilizations, which have their roots in the common human situation. Especially we recognize that the position of the individual within the community exhibits in itself multifarious, often mutually exclusive, aspects»[24].

But this is just a fair guess and no more than that. In fact, Bohr used to shed light on the concept of complementarity by bringing examples taken from the daily life. Consequently, it does not legitimize us to make so brave a connection.

In 1933, Bohr had the opportunity to recall his reading of Kierkegaard during a visit to Jens Rud Nielsen. Rud Nielsen recounted in his paper "Memories of Niels Bohr", written in 1963, the incident as follows:

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[22] Bohr Scientific Manuscripts 1905-1909. "Foredrag over radioaktive Forvandlinger holdt ved Prof. Christiansens Examinatorier", "Lecture on radioactive transmutations given in a Prof. Christiansen's class" from FAVRHOLDT (cit. note 21), p. 56.

[23] SILVANO TAGLIAGAMBE, "Il concetto di realtà fisica e il materialismo dialettico", in M. E. OMELYANOVSKIJ, V. A. FOCK, *L'Interpretazione Materialistica della Meccanica Quantistica: Fisica e Filosofia in URSS* (Milano: Feltrinelli, 1972), p. 71.

[24] BOHR, "The Unity of Human Knowledge", in *Revue de la Fondation Européenne de la Culture*, 1960: 63-66, in *Essays 1958-1962 on Atomic Physics and Human Knowledge* (New York and London: John Wiley & Sons, 1963), 8-16. Reprinted in *The Philosophical Writings of Niels Bohr*, 4 Vols., Vol. 3: Essays 1958-1962 on Atomic Physics and Human Knowledge (Woodbridge, CT: Ox Bow Press, 1987), 8-16, pp. 14-15.

«Knowing Bohr's interest in Kierkegaard, I mentioned to him the translation made by Professor Hollander of the University of Texas, and Bohr began to talk about Kierkegaard: "He made a powerful impression upon me when I wrote my dissertation in a parsonage in Funen, and I read his works night and day", he told me. "His honesty and willingness to think the problems through their very limit is what is great. And his language is wonderful, often sublime. There is of course much in Kierkegaard that I cannot accept. I ascribe that to the time in which he lived. But I admire his intensity and perseverance, his analysis to the utmost limit, and the fact through these qualities he turned misfortune and suffering into something good»[25].

The previous quotation suggests that Bohr had not read anything by Kierkegaard after 1909 and apparently not before. In fact, if Bohr had ever read more than one book by the existentialist philosopher why should he refer to only one specific episode, the one we know from other sources? Nevertheless, this tendency of seeing relevant and unconventional philosophical conceptions of the nineteenth century as they could be transposed "into the realm of natural philosophy" was quite widespread among some historians of science. Holton, while was stressing the absurdity of a direct translation of Kierkegaard's ideas into Bohr's physics, thought that one should allow one self the open-mindedness of reading Høffding and Kierkegaard:

«Through the eyes of a person who is primarily a physicist – struggling, as Bohr was, first with his 1912-1913 work on atomic models, and again in 1927, to "discover a certain coherence in the new ideas" while pondering the conflicting, paradoxical, unresolvable demands of classical physics and quantum physics which were the near-despair of most physicists of the time»[26].

Professor Jan Faye claimed that Holton's proposal is well stated even though it couldn't be sufficient to warrant the conclusion. Faye wrote that in his Danish edition of

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[25] RUD NIELSEN, "Memories of Niels Bohr", *Physics Today*, 1963, 16: 22-30, p. 27.

[26] HOLTON, "On the Roots" (cit. note 20), p. 130.

Høffding's *Den nyere Filosofis Historie* (History of modern philosophy) – written in 1894 – which was from a late friend of him, in his time professor, there is the following marginal note in his hand against the above quotation: "Like 'quanta' in atomic physics". This comparison, which was made many years before anyone had ever considered the existence of a connection between Kierkegaard and Bohr, is a piece of ingenuity. Nevertheless, pointing out the analogy when already acquainted with Bohr's theory of quantum jumps is one thing, but seeing an analogy between Kierkegaard's leaps and the movement of the electron in the hydrogen atom is quite another. What Bohr did was to draw an analogy where Kierkegaard definitely had denied the existence of any. For Kierkegaard, only animate objects could undergo discontinuous changes of state[27]. In fact, Faye sustains in his book that if Bohr possessed any further knowledge of Kierkegaard, in addition to his personal reading of "Stages on Life's Way", it was from having attended Høffding's lectures on Kierkegaard in the spring of 1905. According to Faye, it was just Høffding's interpretation that the "leaps" or jerks were described in a way very similar to Bohr's description of the discontinuous change of atomic systems between stationary states as expressed in the quantum postulates.

In fact, contrary to Kierkegaard, Høffding did believe the "leaps" to exist in inanimate nature.

1.3 The Høffding-Hypothesis

Both Jammer and Holton agreed on the pivotal role played by Høffding in mediating the relationship between Niels Bohr and his interest in philosophy and perhaps this might have influenced him in the successive elaboration of scientific theories.

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[27] Cf. JAN FAYE, *Niels Bohr: His Heritage and Legacy. An Anti-Realistic View of Quantum Mechanics* (Dordrecht: Kluwer Academic Publisher, 1991), pp. 37-38.

Jammer emphasized Høffding's elaboration of Kierkegaard's idea that the traditional speculative philosophy forgot that the originator of the system forms part of the being which is to be explained. According to this interpretation, man cannot conceive of himself as an impartial spectator, as he always remains a participant. Therefore, man regards the delimitation between the objective and the subjective as an arbitrary act. As a consequence, science appears as a determinate activity and truth as a human product. But this conclusion could be misleading if it leads us to consider it as an anticipation of Bohr's conception of reality. Indeed, Bohr was not a subjectivist, as he rejected the idea that the experimental outcome is due to the observer.

Jammer noted that Høffding's point of view on the problem of knowledge seems to presage some conceptual aspects in later quantum mechanics, as, for instance, when he declares:

«In an earlier connection I made use of Schiller's words: "Wide is the brain and narrow is the world"; and now the sentence can be revised: "Wide is the world and narrow is the brain". Knowledge, however rich and powerful it may be, is after all a part of Being; and the problem of knowledge would be soluble, only if Being as a totality (in so far as we can conceive it as such a totality) could be expressed by means of a single one of its parts. In any event, our expression must always remain symbolic, even if our knowledge reaches its climax; it gives us only an extract from a more inclusive whole. Among all the possibilities of thought, only a single one appears in the reality as recognized by us. The reality which we recognize is, however, only a part of a greater whole; - and here we are not in a position to determine the relation between the parts and the whole. An exhaustive concept of reality is not given us to create»[28].

Moreover, Høffding seems to remark that in life only sudden decisions can lead to progress:

[28] HØFFDING, *Filosofiske Problemer* (Copenhagen: Copenhagen University Festskrift, 1902). En. Transl. by GALEN M. FISHER (ed.), *The Problems of Philosophy* (London: McMillan, 1905), pp. 114-15.

«Something decisive occurs always only by a jerk, by a sudden turn which neither can be predicted from its antecedents nor is determined by these»[29].

Høffding called Kierkegaard "the only indeterministic thinker who attempted to describe the leap"[30] but added later: "It seems to be clear that if the leap occurs between two states or two moments, no eye can possibly observe it, and since it therefore can never be a phenomenon, its description ceases to be a description"[31]. Jammer's intention was to sketch some philosophical conceptions, which he attributed indistinctively to Kierkegaard and Høffding, in order to show how these ideas had been translated into the realm of natural philosophy and had stimulated a new approach for epistemology. However, it seems to us that a line of demarcation should be drawn between Kierkegaard and Høffding's conceptions: it concerns what survived of Kierkegaard's teaching in Høffding's philosophy and how it may have influenced Bohr's conceptions of nature. But such a study does not arise from Jammer's book.

In "On the Roots of Complementarity" Gerald Holton wanted to remark the personal sympathy between Høffding and Bohr, giving relevance to the fact that Bohr as a student pointed out some errors in Høffding's book on formal logic, which Høffding welcomed as allowed him to improve his handbook. Moreover, Holton emphasized the importance of Høffding's lectures on philosophy for the young Bohr, who perhaps was struck by Høffding's active interest in the applicability to philosophy of the so-called *philosophierende Naturforscher*, from Copernicus to Newton and from Maxwell to Mach. A great friendship developed between the two that was acknowledged on both

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[29] ID., *Søren Kierkegaard som Filosof* (Copenhagen: P.G. Philipsen, 1892). Then reprinted as *Søren Kierkegaard som Filosof* (Stuttgart: Fromman, 1896), p. 76. From JAMMER, *The Conceptual Development* (cit. note 3), p. 174.

[30] *Ibidem*, p. 83.

[31] *Ibid*.

sides on official occasions such as in the still available correspondence. Last but not the least, Harald Høffding was a friend of Bohr's father too since Niels was a youngster.

As it shall be shown, Jan Faye wrote a distinguished study on Bohr's philosophical background. It is Faye's claim that Høffding's influence on Bohr had more of an indirect nature, that it took the form of inspiring him to comprehend unity in all mankind's search for knowledge. But this and other essential questions deserve a special chapter that will be developed later on.

1.4 The Møller-Hypothesis

Bohr himself – Jammer added – was convinced that Poul Martin Møller, a Danish writer of the early-eighteenth century, with his novel *En Dansk Students Eventyr* (Adventures of a Danish Student) had influenced him. Møller was "an intellectual seeker, almost a wanderer; he had voyaged for two years in the Orient in the guise of a ship's clergyman"[32].

According to the literary historian P.M. Mitchell, Møller represented the beginning of a "bourgeois realism in Danish poetry"[33]. In Møller's novel Bohr found a vivid and suggestive account of the interplay between the various aspects of their position, illuminated by discussions within a circle of students with different characters and divergent attitudes of life. Møller described a conversation between two cousins, one of whom is very soberly efficient in practical affairs, of the type which then, and even now, is known among students as a philistine, whereas the other, called the licentiate, is addicted to remote philosophical meditations detrimental to his social activities. When

[32] LEWIS S. FEUER, *Einstein and the Generations of Science* (New York: Basic Books Publisher, 1974), p. 128.

[33] PHILLIP M. MITCHELL, *A History of Danish Literature* (Copenhagen: Gyldendal, 1957), pp. 123-125.

the philistine reproaches the licentiate for not having made up his mind to use the opportunities for finding a practical job, offered him by the kindness of his friends, the licentiate apologizes most sincerely, but explains the difficulties into which his reflections have brought him[34]:

«My endless inquiries made it impossible for me to achieve anything. Moreover, I get to think about my own thoughts of the situation in which I find myself into an infinite retrogressive sequence of "I"'s who consider each other. I do not know at which "I" to stop as the actual, and as soon as I stop at one, there is indeed again an "I" which stops at it. I become confused and feel giddy as if I were looking down into a bottomless abyss, and my ponderings result finally in a terrible headache»[35].

In his reply the cousin philistine says:

«I cannot in any way help you in sorting your many "I"s. It is quite outside my sphere of action, and I should either be or become as mad as you if I let myself in for your superhuman reveries. My line is to stick to palpable things and walk along the broad highway of common sense; therefore my "I"s never get tangled up»[36].

Léon Rosenfeld – a long-term associated of Niels Bohr – reported that every one of those who came into closer contact with Bohr at the Institute (now the Niels Bohr Institute), as soon as he showed himself sufficiently proficient in the Danish language, was acquainted with Møller's book. Rosenfeld maintained that Bohr's insistence on Møller's novel was a sign of Bohr's rediscovery of the dialectical process of cognition, which had so long been obscured by the unilateral development of epistemology on the basis of Aristotelian logic and Platonic idealism[37].

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[34] Cf. BOHR, "The Unity of Human Knowledge" (cit. note 24), p. 13

[35] *Ibidem.*

[36] *Ibid.*

[37] ROSENFELD, "Niels Bohr's Contribution to Epistemology", *Physics Today*, 1963, 16: 47-54, pp. 47-48.

I would like to point out that it would be interesting to demonstrate that the reading of a novel had started a train of thought leading to the elucidation of the most fundamental aspects of atomic theory and the renovation of the philosophy of science. But all that it could be done is to notice in Møller's novel the idea of an arbitrary dividing line between subject and object that Bohr would subscribe later on. Nevertheless, we are still in the field of the pure similarities. Conversely, it is a fact that Bohr used the situation described in Møller's novel for explaining the complementarity principle. In the address delivered at the Congress held in Copenhagen, October 1960, "The Unity of Human Knowledge", Bohr emphasized that Møller used thoughts and sentiments with reference to mutually exclusive experiences, i.e. in a typical complementary manner.

It is my claim that Bohr's analogy looks like a *post hoc* similitude for representing in words what he had figured in scientific terms.

The student in Møller's novel becomes dizzy when he tries to think about his own thought because precise "thought" and "thought about thought" are complementary.

It resembles to the experimenter who cannot simultaneously show both the wave characteristics and the particle characteristics in a light beam.

Lewis S. Feuer gave a questionable interpretation of Bohr's interests in Møller's novel, as he suggested linking it to the basic generative anxiety in Bohr's unconscious. Feuer held that every philosophical standpoint reflects underlying emotional anxieties and conflicts, together with the attempt to resolve them. According to Feuer, Bohr found the academic philosophies of his time lifeless and uncongenial, thus he sensed that the whole tenor of his life's scientific work was toward resolving the psychological and personal anxieties of which the licentiate in Møllers' novel was the spokesman[38]. In my opinion, Feuer's proposal is questionable because he seems to suggest that Bohr's work on quantum mechanics lies on subjective decisions. Such a conclusion sounds like a mere supposition not supported by sufficient historical proofs.

1.5 The James-Hypothesis

As we have written, it was supposed that William James influenced Niels Bohr. As Holton reported[39], the "discovery" came out "almost by accident", although it suited perfectly with the standpoint of founding the roots of the modern concept of quantum mechanics in Niels Bohr's own experience and, in particular, in his personal interests for humanities. William James, the brother of the famous novelist Henry James, was both a philosopher and a psychologist. His masterpiece is *Principles of Psychology I-II* from 1890. The episode quoted by Holton was the interview arranged by the American Philosophical Society[40] to Bohr in the context of a joint project to assemble the sources for the scholarly study of the history of quantum mechanics. This project was under the general directorship of Thomas S. Kuhn and it spanned several years. Niels Bohr consented to a certain number of interviews, the fifth of which was settled on November 17, 1962 with Kuhn and Aage Petersen, one of Bohr's long-time assistants.

Petersen raised the question of the relevance of the study of philosophy in Bohr's early thought:

Petersen: How did you look upon the history of philosophy? What kind of contributions did you think people like Spinoza, Hume, and Kant had made?

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[38] Cf. FEUER, *Einstein and the Generations* (cit. note 32), pp. 126-31.

[39] HOLTON, "On the Roots" (cit. note 20), p. 119.

[40] Last interview with Professor Niels Bohr, conducted by T. S. Kuhn, Aage Petersen and Erik Rüdinger. 11.17.1962. The Niels Bohr Archive, p. 3.

Niels Bohr: That is difficult to answer, but I felt that these various questions were treated in an irrelevant manner.

P.: Also Berkeley?

NB: No, I knew what views Berkeley had. I had seen a little in Høffding's writings, and I thought it was obvious that so could one do it, but it was not what one wanted.

Thomas S. Kuhn: Did you read the works of any of these philosophers?

NB: I read some, but that was an interest by – oh, the whole thing is coming [back to me]: I was a close friend of Rubin, and, therefore, I read actually the work of William James. William James is really wonderful in the way that he makes it clear – I think I read the book, or a paragraph, called -. No, what is that called? – It is called "The Stream of Thoughts," where he in a most clear manner shows that it is quite impossible to analyze things in terms of – I don't know what one calls them, not atoms. I mean simply, if you have some things ... they are so connected that if you try to separate them from each other, it just has nothing to do with the actual situation. I think that we shall really go into these things, and I know something about William James. That is coming first up now. And that was because I spoke to people about other things, and then Rubin advised me to read something of William James, and I thought he was most wonderful.

TSK: When was this that you read William James?

NB: That may be a little later, I don't know. I got so much to do, and it may be at the time I was working with surface tension, or it may be just a little later. I don't know.

TSK: But it would be before Manchester?

NB: Oh yes, it was many years before.... [L. Rosenfeld remembers clearly that he and Bohr first encountered James together around 1932]. You see, the problem is so difficult, and it may be even irrelevant and immodest to speak so, but I was not interested in philosophy as one generally called it, but I was interested in this special scheme, and that was even not too good.

From the interview one ought to infer that Bohr read James at a very early stage: 1905 approximately. In fact, in 1904 Høffding participated in a congress in St. Louis and during his stay in the United States he visited William James in New Hampshire. It is very likely that after his return to Copenhagen Høffding spoke of James both in his lectures and with his students.

For the sake of clearness, it is worth distinguishing between Bohr's first acquainting with James' psychology and his certain reading of James' *The Principles of Psychology* in 1932.

Rosenfeld doubted that Bohr had read James' book, *The Principles of Psychology*, before that date. Rosenfeld's impression was manifested in the "Interview with Rosenfeld" conducted by Kuhn and John L. Heilbron on July 22, 1963[41]:

TSK: […] one knows that he [Bohr] was a longstanding friend of Rubin. It seems so likely that this suggestion about reading James would have occurred earlier, through Rubin, and that influence, if it was an influence, would have begun at a much earlier stage.

Rosenfeld: No, I don't think so, because Rubin was a friend of Bohr at the university, or at least at that age. And at that age it was not sure at all that Rubin knew already about James. He was only a student, but he might have.

TSK: That was a great big book, *The Principles of Psychology*, and his other writings. If Rubin had been interested in psychology he'd very likely have known about this, and Bohr was obviously talking in this period about sorts of problems of language.

R: When Rubin went into his career as a psychologist, they saw each other occasionally, but they were not such close friends at that, so I can very well imagine that it is only when Bohr came out in public with complementarity, which happened only in 1927, that it struck Rubin that these were his old ideas. Then of course Rubin was well prepared to think of James, but he might not necessarily have seen the similarity at this early stage, so I don't see any incongruence there. All that I can say is that I can remember very distinctly Bohr's enthusiasm. Obviously he read James for the first time then; there was no doubt about it.

TSK: Did you also read it along with him then?

41 Interview with Léon Rosenfeld, conducted with Kuhn and Heilbron 07.22.1963, The Niels Bohr Archive, p. 9.

R: Yes. I had read some small articles by James before, but not his *Principles of Psychology*; I don't remember quite what it was, some small work on pragmatism, a popular exposition of pragmatism.

TSK: But the two of you at this point read James' *Principles of Psychology*?

R: Yes; at least parts of it.

Furthermore, in a letter to Gerald Holton (February 28, 1968) Rosenfeld confirmed that Bohr showed him a copy of James' book only in 1932. It happened when Bohr met again Edgar Rubin after many years. Rubin, the famous psychologist, had been a friend of Bohr since their undergraduate days; and it was he who set up a student club, *Ekliptica*, of which they both were members, whose aim it was to provide a forum for the discussion of Høffding's lectures. So it was supposed that Rubin might have sent the book after their 1932's meeting.

Rosenfeld recalled that Bohr showed great interest for the book and he pointed out to him the passages of "The Stream of Thought".

Bohr was so excited of William James' thought to the extent that Rosenfeld had the impression that that was Bohr's first acquaintance with James' book. Nevertheless, it does not prevent that, in the light of Høffding's visit to James in the autumn of 1904 and his lectures and seminars in the spring of 1905, Bohr had read at least some passages of James' book at the time he was member of *Ekliptica*. As Jan Faye recalls, in the last interview of 1962, Bohr also relates that already in his earliest years as a student he had a theory about free will and determinism as well as about the knowing subject. Thus, he believed there was an analogy between the solutions of multi-valued functions and those of the problems of free will and such matters. According to Faye, it is reasonable to believe that Bohr's interest in these subjects sprang from Høffding's lectures on the psychology of free will, as well as from the conversations in the Bohr family home, where his father and Høffding very probably also discussed free will in the context of the debate concerning mechanism versus vitalism, and thus to believe that he must have been influenced by Høffding's thoughts on these matters and maybe by William James' as well[42].

It is beyond doubt that there are striking analogies between James and Bohr's ideas. It is worth mentioning some quotations of the two authors in order to appreciate the remarkable resemblances of their conceptions.

1) According to James, thought can exist only in association with a specific "owner" of the thought. Consequently, thought and thinker, subject and object are entangled and it is impossible to conceive of the objectivization of thought.
2) We cannot pass over Bohr's sympathetic response: "[…] for objective description and harmonious comprehension it is necessary in almost every field of knowledge to pay attention to the circumstances under which evidence is obtained"[43].

1) According to James, consciousness cannot be concretized or atomized: "consciousness does not appear to itself chopped up in bits. Such words as "chain" or "train", does not describe it fitly as it presents itself in the first instance. It is nothing jointed. It flows. A "river" or a "stream" are the metaphors by which it is most naturally described. In talking of it hereafter, let us call it the stream of thought, of consciousness, or of subjective life"[44]. Nevertheless, there

[42] Cf. FAYE, *Niels Bohr* (cit. note 27), p. 34.

[43] BOHR, "Introduction" to *Atomic Physics and Human Knowledge* (New York: John Wiley & Sons, 1958), p. 2.

[44] WILLIAM JAMES, *The Principles of Psychology* (London: McMillan, 1890), p. 239.

does exist a discontinuous aspect: “the changes, from one moment to another, in the quality of the consciousness are not absolutely abrupt”[45].

2) Paraphrasing Bohr, James would appear to propose a sequence of individual changes between stationary states, with short period of rest between these states – a metaphor that brings to mind Bohr’s 1912-13 quantized atomic model.

Klaus Meyer-Abich reported in his interesting book of 1965, *Korrespondenz, Individualität und Komplementarität*, that among German scientists it was remembered that Bohr was used to quote James and only a few others western philosophers. He also wrote that the philosophical content of Bohr’s interpretation of quantum mechanics was thought out by James already in the years from 1884 to 1890.

To summarize, whereas both Mayer-Abich and Jammer believed that Bohr had read James early enough to be directly influenced, Rosenfeld and Favrholdt sustained that Bohr had independently arrived to analogous conceptions. In a letter to H. P. Stapp from 1972 Rosenfeld wrote:

«With regards to William James, I am quite sure that Bohr only heard of him from his friend, the psychologist Rubin, and from myself in the 30’s. He then expressed enthusiastic approval of James’ attitude, which he certainly felt akin to his own; but it is a fact – a very significant one, I think – that James and Bohr developed a pragmatic epistemology independently of each other»[46].

Stapp quoted this letter on the article titled “The Copenhagen Interpretation” from 1972 in order to introduce another important issue: the influence of pragmatism on the concept of complementarity. He tried to show that the quantum theoretical formalism is to be interpreted pragmatically.

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[45] *Ibidem*, p. 237.

[46] HENRY PIERCE STAPP, “The Copenhagen Interpretation”, *American Journal of Physics*, 1972, 40: 1098-1116, p. 1115.

Stepp went on to maintain that:

«The pragmatic orientation of the Copenhagen interpretation is fixed in the opening words of Bohr's first book: "The task of science is both to extend the range of our experience and reduce it to order […]. In physics […] our problem consists in the coordination of our experience of the external world […]". In our description of nature the purpose is not to disclose the real essence of phenomena but only to track down as far as possible relations between the multifold aspects of our experience»[47].

Moreover, we have to notice that Jammer saw an influence from James even in Bohr's articles from 1928 and 1929, "The Quantum of Action and the Description of Nature":

«The unavoidable influence on atomic phenomena caused by observing them here corresponds to the well-known change in the tinge of psychological experiences which accompanies the direction of attention to one of the various elements»[48].

This can be read in many ways. In fact – as I shall return to this below – Bohr knew a lot about psychology through Rubin, and he might have considered the problems of perception as already dealt with both in Rubin's doctoral thesis and in the discussions they had together in the "Ekliptica club" some years before, as it is confirmed by Bohr's personal correspondence.

1.6 The Emergence of Continuity

Returning to the Jammer/Rosenfeld's "disagreement", Holton, in his "The Roots of Complementarity", reputed Rosenfeld's alternative as regards Bohr's autonomous way of approaching the concept of complementarity as more interesting, though difficult one, "for hints that here may be a place to attack the haunting old question why and by

[47] *Ibidem*.

[48] BOHR, "Wirkungsquantum und naturbeschreibung", *Naturwissenschaften*, 1929, 17: 483-486, translated into English as "The Quantum of Action and the Description of Nature", in *Atomic Theory* (cit. note 1), 92-101, p. 100. Reprinted in *NBCW*, 6 (cit note 1), 208-217, p. 216.

what mechanism the same themata attain prominence in different fields in nearly the same periods"[49]. One could hypothesize that Bohr was brought to analogous thought by, for instance, the reflection on the concept of multiform function or Riemann's surface.

Nonetheless, Holton rightly noted that the analogy between James' "The Stream of Thought" and Bohr's concept of complementarity is striking, apart the question of the genetic connection between the two uses of the same word. Thus he suggested that a James' influence on Bohr must be occurred if we consider the importance that Høffding recognized to James' works among all the philosophers and scientists discussed during his lectures.

Perhaps the way out to such a riddle could be found by heeding the thematic origin of Niels Bohr's rational generalization thesis. Max Jammer too noted that Bohr regarded the older quantum theory as a rational generalization of classical mechanics. Consequently, its metaphysical and epistemological foundations were generally held to be those of classical physics. For this reason – Jammer wrote – "no specific philosophical study of the conceptual foundations of the older quantum theory seemed to be required with the possible exception of Bohr's insistence on methodological considerations"[50].

As a matter of fact, philosophers of science before mid-twenties regarded quantum theory as of no interests.

I want to emphasize that it is just Bohr's insistence on methodological considerations the key to clarify his conception of the quantum theory as a rational generalization of classical mechanics. As Holton noted – there is a methodological strategy underneath

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[49] HOLTON, "On the Roots" (cit. note 20), p. 123.

[50] JAMMER, *The Conceptual Development* (cit. note 3), p. 166.

Bohr's older quantum theory of emphasizing conceptual conflict as a necessary preparation for its solution that culminated, fourteen years later, in the announcement of the complementarity principle[51]. In the meantime, Bohr formulated a proposal of reconciliation between classical and quantum mechanics, the correspondence principle.
Bohr was still searching for a resolution of the opposites by developing a principle for which classical physics becomes the limiting case of quantum physics. As Holton wrote:

«In this situation the quantum of action relative to the energies involved is effectively zero rather than having a finite value, and the discreteness of individual events is dissolved, owing to the large number of events, in an experienced continuum»[52].

In other words, this striving after the reconciliation of the two physics is the main characteristic of Bohr's method that should lead us to reconsidering the Aristotelian maxim *Natura non facit saltus* – that stresses the element of continuity – on the gnoseological level, rather than the mere physical one, for which Bohr would emphasize the element of discontinuity.
It is worth remarking that the reconciliation of classical with the quantum physics was in Bohr's view the return to the condition of continuity. In fact, Bohr sustained that the breakdown of visualisability in microphysics was primarily due to the failure of the condition of continuity. Space, time and causality are forms of perceptions according to Bohr. Moreover, he pointed out that the causal spatio-temporal description of classical physics presupposes continuity. Generally speaking, it is the organization of our

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[51] HOLTON, "On the Roots" (cit. note 20), p. 114.
[52] *Ibidem*, p. 115.

ordinary sensory experience, i.e. the ordinary language itself that presupposes continuity.

The forms of perception are the means by which we organize our perceptual experience, and they also determine the conceptual structure of classical physics. It is also worth noting that Harald Høffding stressed that the forms of perception and the categories of understanding are essentially equivalent to the requirement of continuity. One of the main themes of his philosophy in fact is a dualism of continuity and discontinuity, which underlies almost every philosophical problem.

As is known, in 1913 Bohr put forward a mathematical model of the atom, which provided the first theoretical support for Rutherford's atomic planetary model for explaining the emission spectrum of the hydrogen atom. In short, Bohr's model consists of the application to Rutherford's of the two famous postulates: that an atomic system is stable only in a certain set of states and that the possibility for the atom to absorb/ emit radiation is determined by a law according to which the energy of the radiation is given by the energy difference between two stationary states being equal to hv where h is Planck's constant[53] and v is the frequency. As a consequence, Bohr's semi-classical

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[53] In 1900 Max Planck discovered that the radiation spectrum of black bodies occurs only with discrete energies separated by the value hv where v is the frequency and h is a new constant, the so-called Planck constant. According to classical physics the intensity of this continuous radiation would grow unlimitedly with growing frequencies, resulting in what was called the ultraviolet catastrophe. But Planck's suggestion was that if black bodies only exchange energy with the radiation field in a proportion equal to hv that problem would disappear. The fact that the absorption and the emission of energy is discontinuous is in conflict with the principles of classical physics. A few years later Albert Einstein used this discovery in his explanation of the photoelectric effect. He suggested that light waves were quantized, and that the amount of energy which each quantum of light could deliver to the electrons of the cathode, was exactly hv. The next step came in 1911 when Ernest Rutherford performed some experiments shooting alpha particles into a gold foil. Based on these results he could set up a model of the atom in which the atom consisted of a heavy nucleus with a positive charge surrounded by negatively charged electrons like a small solar system. Also this model was in conflict with the laws of classical physics. According to classical mechanics and electrodynamics one might expect that the electrons orbiting around a positively charged nucleus would continuously emit radiation so that the nucleus would quickly swallow the electrons.

model was anomalous, as it introduced elements of discontinuity and indeterminism *almost* foreign to classical physics. Moreover, as Merle A. Tuve noted, Bohr's atom was "quite irrational and absurd from the viewpoint of Newtonian classical mechanics and Maxwellian electrodynamics […] Various mathematical formalisms were devised which simply 'described' atomic states and transitions, but the same arbitrary avoidance of detailed processes, for example, description of the actual process of transitions were inherent in these formulations"[54]. Jan Faye outlined the main characteristics of Bohr's model as follows[55]:

1. Apparently not every point in space was accessible to an electron moving around a hydrogen nucleus. An electron moved in classical orbits, but during its transition from one orbit to another it was at no definite place between these orbits. Thus, an electron could only be in its ground state (the orbit of lowest energy) or an excited state (if an impact of another particle had forced it to leave its ground state).
2. It was impossible to predict when the transition would take place and how it would take place. Moreover, there were no external (or internal) causes that determined the "jump" back again. Any excited electron might in principle move spontaneously to either a lower state or down to the ground state.
3. Rutherford pointed out that if, as Bohr did, one postulates that the frequency of light *v*, which an electron emits in a transition, depends on the difference between the initial energy level and the final energy level, it appears as if the

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[54] MERLE A. TUVE, "Physics and the Humanities – The Verification of Complementarity", in C. P. HASKINS (ed.), *The Search for Understanding* (Washington D. C.: Carnagie Inst., 1967), p. 46.

[55] Cf. FAYE, "Copenhagen Interpretation of Quantum Mechanics", *Stanford Encyclopedia of Philosophy*, 2008.

electron must "know" to what final energy level it is heading in order to emit light with the right frequency.

4. Einstein made another strange observation. He was curious to know in which direction the photon decided to move off from the electron.

Moreover, Bohr continued to pursue the reconciliation of quantum and the classical physics in spite of the new quantum theory would violate some of the basic ontological principle on which classical physics rests:

1. *The principle of space and time*, i.e., physical objects (systems) exist separately in space and time in such a way that they are localizable and countable, and physical processes (the evolution of systems) take place in space and time;
2. *The principle of causality*, i.e., every event has a cause;
3. *The principle of determination*, i.e., every later state of a system is uniquely determined by any earlier state;
4. *The principle of continuity*, i.e., all processes exhibiting a difference between the initial and the final state have to go through every intervening state;
5. *The principle of the conservation of energy*, i.e., the energy of a closed system can be transformed into various forms but is never gained, lost or destroyed.

For this reason, in retrospect, it was remarked how "courageous" Niels Bohr's 1912-13's work was. Notwithstanding the conceptual and scientific contradictions, Bohr had the courage to carry his atomic model through, as he stressed these conflicts from the beginning. Indeed, the explanation of the spectral lines, which were the main result he had achieved, appeared to represent an addendum in his own work. As Holton noted: "his interest was precisely to examine the area of conflict between the conceptions of ordinary electrodynamics and classical mechanics on the one hand and quantum physics on the other"[56]. The principle of correspondence seems to be informed by such a spirit of "conceptual reconciliation" that pervaded Bohr's program since the beginning. Bohr and his collaborators saw in the principle of correspondence the hope to connect the discontinuous processes of the atoms with the continuous character of the radiation field.

In more general terms, they saw in such a principle the possibility to obtain the harmonious re-incorporation of the quantum of action into a new consistent theory.

The peculiar process they followed deserves a specific study, as we are just in sight of the question of what makes scientific discovery so unrepeatable an event. As I see it, it was just the weight of the philosophical background that moulded Bohr's aim to pursue that harmony or unity between macrocosm and microcosm, possibilities of observation and definition, theory and reality, and whose necessary condition was the above-mentioned concept of continuity.

[56] HOLTON, "On the Roots" (cit. note 20), p. 113.

Chapter 2

The Foundation of Metaphysical Heuristic

2.1 Some Introductory Considerations

Neo-empiricist Philosophy of Science regarded knowledge's validity as man's capacity of discerning science, *i.e.* the most respectable kind of knowledge, from superstition, ideology and pseudoscience[1].

Therefore the problem of demarcation became of social and political relevance.

Nevertheless, post-empiricist epistemologies of Sixties and Seventies hardly criticized the criterion of demarcation, which the history of science's development also suggested to abandon.

Imre Lakatos' Methodology of Scientific Research Programs [MSRP] was built on insufficient explanations concerning the hard core and the negative heuristic. In fact, Lakatos seemed to introduce two meanings of the latter [methodological decision and metaphysical "conceptual picture"] and an incomplete definition of the former. Furthermore, he did not clarify the origin of the so-called metaphysical value of the positive heuristic. In light of the concepts of "metaphysical heuristic" and "inter-

[1] IMRE LAKATOS, "Science and Pseudoscience", in JOHN WORRAL, GREGORY CURRIE (eds.), *Philosophical Papers I – The Methodology of Scientific Research Programs* (Cambridge, GB: Cambridge University Press, 1978), p. 1. This paper was written in early 1973 and was originally delivered as a radio lecture. It was broadcasted by the Open University on June 30, 1973.

translatability" I propose to merge the two negative heuristic's meaning. Therefore, both the positive and the negative heuristic are methodological rules deriving directly from a common metaphysical principle. Consequently, the traditional distinction between internal and external history becomes untenable.

I shall start from a critic of Lakatos' MSRP in order to clarify the origin of Niels Bohr's methodological precepts at the basis of his research program. As a matter of fact, Lakatos gave the first epistemological account for the role played by heuristic within a research program. Nevertheless, beyond his merits as one of the most influent philosophers of science in the twentieth century, he was not able to clarify the metaphysical role of heuristic, as he tried to confine metaphysics in the world of irrationality. Zahar provided the so-called metaphysical heuristic with a new statute but without attempting to define the meaning of the "genetic material", which scientific theories originated from. In order to fill Zahar's gap I shall make use of Gerald Holton's thematic analysis and Michael Friedman's role of philosophical reflection.

2.2 Introduction and Criticism to Lakatos' Philosophy of Science

According to Lakatos's account, the typical unity of science is not an isolated hypothesis, but rather a *research program* consisting of a developing series of theories, characterized by a continuity which evolves from a proto-program adumbrated at the research's start. Lakatos laid down rules in order to appraise programs, which he divided in either progressive or degenerating ones.

As it is well known through his special jargon, a program is *theoretically progressive* if each modification leads to new unexpected predictions. It is *empirically progressive* if at least some of these novel predictions are corroborated[2].

The program's structure consists of three main components[3]:

a) the "Hard Core".
b) The "Negative Heuristic".
c) The "Positive Heuristic".

It is worth noting that Lakatos gave also ambiguous definitions of heuristic and incomplete descriptions of the hard core.

According to his paper, "Falsification and the Methodology of Scientific Research Programs"[4], one apprehends that every research program may be characterized by its own hard core, whose negative heuristic forbids us to refute it by means of a methodological *ukase*. Therefore Lakatos affirmed that the negative heuristic *specifies* the hard core of a program.

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[2] LAKATOS, ELIE ZAHAR, "Why Copernicus's Program Superseded Ptolemy's" in *Philosophical Papers I* (cit. note 1), p. 179. This paper was written with Elie Zahar in 1972-73. It was first published as Lakatos and Zahar, "Understanding Toulmin", *Minerva*, 1976, 14: 126-143. Lakatos gives the following account of the paper's history: "This talk was first given at the Quincentenary Symposium on Copernicus of the British Society for the History of Science, on January 5, 1973. The paper is the result of joint efforts of the co-authors, but it is narrated in the first person by Imre Lakatos. Previous versions were criticized by Paul Feyerabend and John Worral".

[3] LAKATOS, "Falsification and the Methodology of Scientific Research Programs" in *Philosophical Papers I* (cit. note 1), p. 47. This paper was written in 1968-69 and was first published as LAKATOS, ALAN MUSGRAVE, *Criticism and the Growth of Knowledge* (Cambridge, GB: Cambridge University Press, 1970).

[4] *Ibidem*, pp. 48-49.

Elie Zahar interpreted Lakatos's account by explaining that the hard core, in order to be protected against experimental refutation, ought to be metaphysical in Popper's sense. A conclusion that Lakatos did not clearly draw[5].

The Hungarian philosopher rather appealed to history of science's examples, such as the three laws of motion and the law of gravitation in Newton's research program, for explaining the hard core's structure[6].

Both Lakatos and Zahar in a 1972-73 paper stressed the point that the demarcation between hard core and heuristic is frequently a matter of convention as they suggested in their historical analysis of the Pythagorean-Platonic programs. The key principle of the two-programs was that all astronomical phenomena should be regarded as a combination of uniform circular motions since "heavenly bodies" are perfect. This principle represented the heuristic of both the programs and it was primary with respect to the hard core [secondary] that should indicate where the centre of the universe lies[7].

This remark gives rise to an ambiguity on the hard core's meaning.

As mentioned above, it might seem that a program's hard core is *specified* by the negative heuristic, although this "specification" does not allow us going back historically to the source of the hard core's statements.

In a footnote to his masterpiece Lakatos pointed out that «the actual hard core of a program does not emerge fully armed like Athena from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error»[8].

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[5] ZAHAR, *Einstein's Revolution. A Study in Heuristic* (Chicago and La Salle, IL: Open Court Publishing Company, 1989), p. 21.

[6] LAKATOS, "Falsification and" (cit. note 3), p. 48

[7] LAKATOS, ZAHAR, "Why Copernicus's" (cit. note 2), p. 180.

[8] LAKATOS, "Falsification and" (cit. note 3), p. 48 [ft. 4].

I think that a thorough study of the hard core's structure should take the above-mentioned assertion into account.

Some others interesting objections to Lakatos's theory of science had been raised by Alan Musgrave[9], who asked why scientists should specify in advance a program's hard core. In fact, before 1850, Newtonians rarely treated Newton's law of gravitation as a part of it. Musgrave claimed that there are no reasons to rule it out in advance despite the difficulties of producing a progressive theoretical problem-shift by changing central assumptions. Musgrave thought that Lakatos was overcautious in not recommending any rule for choice between competing research programs. Why not say, that on the whole, the scientific community should devote more resources to progressive as opposed to degenerating research programs, Musgrave added.

Karl Popper[10] and John Watkins[11] proposed arguments concerning the inter-translatability between metaphysics and heuristic in a way that might have worked out retrospectively the problem of the regulative role of negative heuristic.

Elie Zahar wrote enlightening words about ontology and metaphysics in scientific theories, which I shall refer to.

Returning to Lakatos' MSRP, a research program may be defined as a set of methodological rules otherwise said heuristic: *negative heuristic* «tells us what path of research to avoid», *positive heuristic* «what path to pursue»[12].

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[9] MUSGRAVE, "Method or Madness" in R. S. COHEN, P. K. FEYERABEND, M. WARTOFSKY (eds.) *Essays in Memory of Imre Lakatos* (Dordrecht, NL: Reidel, 1976).

[10] KARL. R. POPPER, *Logik der Forschung* (Vienna: Springer, 1934), English translation, *The Logic of Scientific Discovery* (London: Hutchinson, 1959, 1966, 1968), § 11 [1934]: "Not a few doctrines which are metaphysical…could be interpreted as typical hypostatizations of methodological rules".

[11] JOHN W. N. WATKINS, "Confirmable and Influential Metaphysics", *Mind*, 1958, 67: 344-365.

[12] Lakatos, "Falsification and" (cit. note 3), p. 48.

Lakatos brings to our attention “Cartesian metaphysics” as an example of a particular research program, that is the mechanistic theory, according to which the universe is a “huge clockwork” and a *system of vortexes*. It played both the positive and the negative heuristic’s role[13].

It discouraged work on theories incompatible with it [Newton’s theory of action at a distance], and it encouraged work on auxiliary hypotheses, which might have saved it from apparent counterevidence [Keplerian ellipses].

But I want to emphasize the negative heuristic’s meaning. Lakatos termed it the *methodological decision* «not to allow refutations to transmit falsity to the hard core as long as the corroborated empirical content of the protecting belt of auxiliary hypotheses increases»[14]. The above-mentioned definition, from my point of view, does not throw light on the point because the negative heuristic is also the metaphysical expression of a “conceptual picture” which affects the research’s path. As it is exampled by the heuristic role of the mechanistic theory of the universe in the seventeenth Century.

What is the epistemological statute of such a heuristic?

One may hypothesize that the two meanings of negative heuristic could be bridged by considering such a methodological decision as metaphysics in the sense that the decision is inspired either by scientists’ (personal) convictions or by “philosophical” doctrines. Thus, metaphysics would be nothing but scientists’ “general ideas” about science: determinism, indeterminism, atomism et cetera.

But such metaphysics could not be *internal* to a research program.

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[13] *Ibidem*.

[14] *Ibid*., p. 49.

The positive heuristic's role is apparently more explicit than the negative because it constructs the belt, which protects the irrefutable nucleus of the program from the counter-evidence. The positive heuristic is «a partially articulated set of suggestions or hints on how to change, develop the "refutable variants" of the research program, how to modify, sophisticate, the "refutable" protective belt»[15].

It rules the program's "research policy" but it is a "model" too, a kind of "theoretical cabinet" in which initial conditions, together with corroborated observational theories, allow the scientist to develop the program by adopting an ad hoc strategy.

The positive heuristic, according to Lakatos, may be formulated also as a metaphysical principle: when a research program is to degenerate, a *creative shift* in its positive heuristic may reinvigorate it.

Lakatos defined the positive heuristic more flexible than the negative therefore it is better – he affirmed – to separate the hard core from the metaphysical principles expressing the positive heuristic. An assertion which does need to be clarified.

2.3 The metaphysical Structure of the Hard Core

The hard core of a program becomes unassailable only through a methodological decision[16], from which it follows that any falsifier of the hard core can be rejected. The hard core forms the main defining characteristic of the research program: whenever the hard core is rejected, the program as a whole is rejected too.

It has to be remarked that every proposition, independently of any stipulation, either is or is not metaphysical. In particular, if the hard core is un-falsifiable, it must be metaphysical in some absolute sense.

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[15] *Ibid.*, p. 50.

[16] *Ibid.*

As Zahar suggested[17], the requirement that the hard core be *per se* irrefutable establishes a close correlation between MSRP and Popper's notion of a metaphysical research program.

According to classical empiricist epistemology [Hume, Mach] an entity was said to be metaphysical if it could be neither observed nor operationally determined. Thanks to Bridgman's contribute, a proposition could then be termed metaphysical in a secondary sense: if it involves "occult" entities.

By contrast, Popper affirmed that metaphysical propositions possess no empirically decidable consequences.

According to falsificationism, the difference between physics and metaphysics is no longer the unbridgeable gap between sense and nonsense, which "The Vienna Circle" upheld, but the simple difference between the testable and the irrefutable. Popperian philosophy can be thus said to have rehabilitated metaphysics.

After taking cognizance of Tarski's truth-definition – Zahar explains[18] – Popper realized that syntactically well-formed sentences could be regarded true-or-false without necessarily having to be effectively verifiable or even refutable; more particularly: a metaphysical thesis ought by right to possess a definite, though possibly un-decidable truth-value.

In this sense Popper affirmed the existence, within the history of science, of *metaphysical research programs*. By contrast, Lakatos claimed that metaphysics is internal to research programs.

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[17] ZAHAR, *Why Science needs Metaphysics. A Plea for Structural Realism* (Chicago, IL: Open Court, 2007), p. 139.

[18] *Ibidem*, p. 141.

Zahar makes clear the thesis that the hard core K of a research's program RP is a metaphysical principle entailed by every member of the program. So the sustained empirical success of a RP can lend some support to its hard core. Conversely, the repeated failure of a RP must negatively reflect on its metaphysics[19]. For example: as regards Newton's Absolute Time Hypothesis [ATH] Zahar showed[20] how the addition law of motion depends on the assumed absoluteness of all temporal distances between events.

It is Zahar's claim that heuristic must be connected to the core, *i.e.* the metaphysics underpinning the program, in order for the heuristic to be appraised.

He added that *certain* components of the hard core have prescriptive counterparts, which can in turn be translated into meta-statements about scientific hypotheses. Therefore an ontological thesis will impose constraints on theories. Such constraints form heuristic, which Zahar termed metaphysical. It follows that the distinction between negative and positive heuristic is not as sharp as Lakatos affirmed.

Zahar's thesis is consistent with the example of Quantum Mechanics, which continues to support indeterminism at the micro-level and to undermine the classical notion of causality.

In addition it is worth remarking that, for instance, Bohr's quantum conditions[21] are ontologically different, in degree rather than in kind, from indeterminism, although both are metaphysical.

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[19] *Ibid.*

[20] *Ibid.*, p. 140.

[21] LAKATOS, "Falsification and" (cit. note 3), p. 56.

The question is how to assess the metaphysics underlying the methodological decision made at the RP's start, for which the hard core's statements become irrefutable.

As mentioned above for Cartesian metaphysics, such ontological theses, *e.g.* determinism, indeterminism, mechanism, etc, could play a vital role in deciding the direction of *Modus Tollens*'s arrow. I suppose they would act on the hard core as "metaphysical heuristic".

2.4 On the logical structure of Metaphysical Heuristic

Following the conclusions of the last paragraph, I should define the concept of "metaphysical heuristic". I owe this term to Zahar but I shall use it in a different and perhaps improper meaning (see above in the previous page).

Zahar gives a rational account of heuristic[22] as he showed by means of a few examples from the history of science that the logic of scientific discovery is not inductive but deductive. If this claim holds, the "irrational" character of the context of discovery will be not different from that of the context of justification; for in both cases, the methods used turn out to be largely deductive.

Against Lakatos, he defends the view that heuristic possesses «no special status halfway between psychology and logic»[23], for he thinks that deduction constitutes the most important moment in the process of invention.

Zahar claims that heuristic principles are connected with the birth of science proper. They are as stable as «the genetic material of which we are made»[24]. They arose from confrontations with different problem-situations.

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[22] ZAHAR, *Why Science* (cit. note17), pp. 149-53.

[23] *Ibidem*, p. 150.

The heuristic of any RP «is determined by the *coherent choice* it operates among these principles»[25].

An RP would evolve through the application of its heuristic to existing hypotheses, some of which might initially have belonged to commonsense knowledge: the Identity Principle, the Unity Principle, the Principle of the Proportionality of the Effect to its Cause, the Principle of Sufficient Reason, the Rejection of Conspiracy Theories.

In light of Watkins' article, "Confirmable and Influential Metaphysics", I shall assess the logical structure of both "made-by-decision" metaphysical statements and the so-called *haunted-universe doctrines*.

Moreover, I hypothesize that the concept of inter-translatability may explain the regulative, heuristic, role of metaphysics within research programs. Considering the classification[26] of the statements proposed by Watkins, there are four degrees of empirical decidability:

1) Level – 1: Circumscribed existential statements. Falsifiable and verifiable, *e.g.* "There is a car in my garage".
2) Level – 2: Universal empirical hypotheses. Falsifiable but not verifiable, *e.g.* "All metals expand if heated".
3) Level – 3: Un-circumscribed existential statements. Un-falsifiable but verifiable, *e.g.* "There exists a metal which does not expand if heated".
4) Level – 4: "All-and-some" statements. Unverifiable and un-falsifiable, *e.g.* "For all metals there exists some acid which will dissolve them".

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[24] *Ibid.*, p. 151.

[25] *Ibid.*

[26] WATKINS, "Confirmable and" (cit. note 11), pp. 345-46.

Lakatos's sketch[27] of Niels Bohr's RP illustrates how a program may progress even on inconsistent foundations following the decision to isolate certain conditions in order to develop the program.

Therefore, Bohr decided, for the time being, to ignore the inconsistencies between Rutherford's atomic planetary model and the Maxwell-Lorentz theory.

He proposed five postulates as the hard core of his program.

i) That energy radiation [within the atom] is not emitted (or absorbed) in the continuous way assumed in the ordinary electrodynamics, but only during the passing of the systems between different "stationary" states.

ii) That the dynamical equilibrium of the systems in the stationary states is governed by the ordinary laws of mechanics, while these laws do not hold for the passing of the systems between the different states.

iii) That the radiation emitted during the transition of a system between two stationary states is homogeneous, and that the relation between the frequency v and the total amount of energy E is given by $E = hv$, where h is Planck's constant.

iv) That the different stationary states of a simple system consisting of an electron rotating round a positive nucleus are determined by the condition that the ratio between the total energy, emitted during the formation of the configuration, and the frequency of revolution of the electron is an entire multiple of ½ h. Assuming that the orbit of the electron is circular, this

[27] LAKATOS, "Falsification and" (cit. note 3), pp. 55-68.

assumption is equivalent with the assumption that the angular momentum of the electron round the nucleus is equal to an entire multiple of $h/2\pi$.

v) That the "permanent" state of any atomic system, *i.e.* the state in which the energy emitted is maximum, is determined by the condition that the angular momentum of every electron round the centre of its orbit is equal to $h/2\pi$.

By the comparison of Bohr's postulates with Watkins' classification, it follows that the former are second level statements *i.e.* universal empirical hypotheses which have been rendered metaphysical only by decision.

According to Watkins' paper, "haunted-universe doctrines" are unempirical in the sense they are compatible with every conceivable finite set of observation statements. They are factual because there are empirical theories with which they will not be compatible. They match with the fourth level "all-and-some" statements.

Classical examples of "all-and-some" doctrines, according to Watkins's account, are determinism, historicism, the theological variant of historicism, mechanism and its variants. For instance: "Every event has its cause" is an "all-and-some" statement deriving directly from mechanism.

Lakatos took Watkins's position into account in his 1972-73[28] paper, but he had formerly by-passed the "external" role of metaphysics in science by circumscribing the adjective "metaphysical": «I only talk about *scientific* research programs whose hard core is irrefutable not necessarily because of syntactical but possibly because of methodological reasons which have nothing to do with logical form. […] Separating sharply the *descriptive problem* of the psychological-historical role of metaphysics from

[28] LAKATOS, ZAHAR, "Why Copernicus's" (cit. note 2), p. 181 [ft. 1].

the *normative problem* of how to distinguish progressive from degenerating research programs. […]»[29].

Nevertheless Lakatos, as I see it, failed to establish the foundations of such *methodological reasons*, which express metaphysical content of the fourth level (see above on the negative heuristic meanings).

The inter-translatability between metaphysics and heuristic could bridge the gap: "all-and-some" doctrines may «exhort the scientist to look for a certain kind of thing while appearing to inform him that that sort of thing is there to be found»[30]. Universal doctrines clash with certain kinds of falsifiable hypotheses and so prohibit their construction.

I would highlight that the function of metaphysical heuristic is twofold with respect to Watkins's influential metaphysics. Indeed, metaphysical heuristic in its negative version would affect the research program in the very early stages of its evolution. Metaphysical heuristic in its positive double-version ["protective mechanism" and "metaphysical principle"] lets it develop on.

I daresay that inter-translatability is thus consistent with the formulation of the positive heuristic as metaphysical (heuristic) principle. For instance: the hypothesis that «the planets are essentially gravitating spinning-tops of roughly spherical shape»[31], as Newton's program may be formulated, is certainly metaphysical because the planets, for example, have also electromagnetic characteristics. Ultimately, Lakatos seems to prove us right. In fact, on page 51 of his "Falsification and the Methodology of Scientific

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[29] LAKATOS, "Falsification and" (cit. note 3), p. 96.

[30] WATKINS, "Confirmable and" (cit. note 11), p. 356.

[31] LAKATOS, "Falsification and" (cit. note 3), p. 51.

Research Programs" he formulates the "positive heuristic of a research program" as "metaphysical principle" in its double role of creative and protective device. While on page 58, explaining the twofold role of Bohr's correspondence principle, he clearly states that the correspondence principle functioned both as an important "heuristic principle" and a defence mechanism. This is to say that he equated the term "metaphysical principle" to the term "heuristic principle", from which we get the concept of metaphysical heuristic. On the one hand, the correspondence principle on behalf of metaphysical heuristic exerted "creative functions" and suggested many new scientific hypotheses, which, in turn, led to novel facts, especially in the field of the intensity of spectrum lines. On the other hand, as a defence mechanism, the correspondence principle endeavoured to utilize to the utmost extent the concepts of the classical theories of mechanics and electrodynamics, in spite of the contrast between these theories and the quantum of action.

Positive and negative heuristic, as regards their common metaphysical origin, would be different faces of the same medal. Metaphysics, by playing both a decisional role at the RP's start and a guiding one during its evolution, is not influential but rather immanent.

Therefore the difference between *internal* and *external* history, i.e. between rational and irrational factors, is not as sharp as Neo-Popperians wish.

2.5 The Thematic Approach

Zahar's arguments are fruitful because they permit bypassing the schematic distinction between positive, negative heuristic and hard core. Nevertheless, they are also incomplete in the sense that the relation between metaphysics and the "existing hypotheses" belonging to common sense knowledge has not been defined as well as it has not been the origin of such hypotheses: what does it mean common sense knowledge? Are they *a priori* pure intuitions? In short, it is not still clear at all what is the place of metaphysics with respect to RP.

For these reasons it was necessary to make use of the concept of inter-translatability between metaphysics and heuristic. Even so we cannot pass over the importance of Zahar's work that brought to light the independent role of metaphysics within RP: i.e. not as a mere consequence of a methodological decision.

After having ascertained that *science needs metaphysics*, we are about to define the function exerted by metaphysics in science in some practical terms.

Gerald Holton wrote enlightening words on the study of the thematic origins and elements in scientific works, with particular reference to the personal presuppositions. Almost five decades ago he published for the first time on the concept of thematic analysis of scientific thought (in Eranos-Jahrbuch of 1962). The approach laid out there and in the book published by Harvard University Press in 1973 (revised edition, 1988) has been at the heart of much of his subsequent research in the history of science, as well as having been taken up by others.

I am referring to Holton's thematic analysis because following Watkins he was the first to describe in historical terms the so-called third dimension of science, characterized by presuppositions that are neither confirmable nor falsifiable. They do not arise from the data or the theory but are imposed on them by the scientist from the outset. Holton distinguished the third dimension of science from the phenomenic one as well as from the logical-analytical system, which form the x-y axes of the plane of discourse in science. The two-dimensional view of science has his defects because it does not explain, for instance, why at any given time the choice of problems or the reception of theories may be strikingly different among individuals or like-minded groups who face the same corpus of data. That was the case of Einstein and Bohr.

Holton noted that thematic analysis is not an ideology, a school of metaphysics, a plea for irrationality, an attack on the undoubted effectiveness of empirical data and experimentation, or a means for teaching scientists how to do their job better. It is rather useful for explaining the willingness of the scientists to adopt what can only be called a suspension of disbelief about the possible falsification of their hypotheses that emerges from the data. Consequently, Holton's account of themata [singular, thema; it corresponds to the Greek for "that which it laid down"; "proposition"; "primary word"] matches with the concept of negative heuristic as "conceptual picture". I would say that such themata might exert metaphysical-heuristic functions within the theories' development. Even though it should be necessary to set the origin of each singular thema.

If we decided to apply the thematic approach to Zahar's scheme, it would seem that we tried to connect each thema to its original or "a priori" principle. But it is worth remarking that among the concepts that might be confused with themata, the most obvious is what Immanuel Kant called "Categories". Apart from other differences, Kant's "Categories" were to be accepted as "pure concepts of the understanding, which apply a priori to objects of intuition in general".

Holton warns us against confusing other concepts with thematic propositions. Examples are metaphors and related notions such as mental models, frames, and schemata. The metaphoric imagination is a lively component of science such as the visual and the thematic. But as even its original Greek definition implied, metaphor, unlike themata, serves the traditional function of making conceptual connections between selected similarities; moreover, they are in principle infinite in numbers. On the contrary, many themata are widely shared, and in science such as physics they are and have been few in number.

Another potential confusion involves the concept of “paradigm” that refers primarily to a social phenomenon in the scientific profession. By contrast, a thema is found in individual work, as part of an individual’s spectrum of themata that no one else may have accepted *in toto*. Moreover, themata are found to be finite in number and generally of long duration, and so accentuate the longevity and evolutionary nature of scientific advance.

The main point of interest of Holton’s account rises from his immersion in Einstein’s correspondence and manuscripts. Through the reading of Einstein’s drafts of his work and copies of his correspondence, Holton came to understand that, in addition to his attention to data and logical/analytical tools (which Einstein himself referred to as the “empirical” and the “rational” aspects), a mainspring of his work was often a set of fiercely held presuppositions, what he had identified as thematic elements. Holton enumerated the themata guiding Einstein’s theory construction: the primacy of formal rather than materialistic explanation; unity or unification; logical parsimony and necessity; symmetry; simplicity; causality in the classical sense; completeness in the subordination of every phenomenon under the respective theory; the continuum; and of course constancy and invariance.

These thematic components can sometimes be observed also in the public papers, although most scientists take care to keep such metaphysical aspects of their works out of view. Holton pointed out that the public sources offer only a partial view of the history of science:

«In all these cases, one may discover that during the nascent, "private" period of work, some scientists, consciously or not, use highly motivating, very general thematic presuppositions. But when the work is then proposed for entry into the "public" phase of science, these motivating aids tend to be suppressed, and even disappear from view. Even though thematic notions arise from a deep conviction about nature, on which the initial proposal and eventual reception or rejection of one's best work may be based, they are not explicitly taught, and they are not listed in the research journals or textbooks. That has certain advantages, insofar as silence about personal motivations and thematic preferences avoids any deep, un-resolvable, disputes in the public phase. Consensus is more easily reached if thematic elements are kept out of sight»[32].

I would add in addition to Holton's account that if we focused the attention on scientist's personal convictions or individual presuppositions, we would be able to look at the public papers with different eyes because the motivations underpinning the scientific discovery would emerge also from the public sources. Even the greatest scientist would appear with his uncertainties, contradictions, doubts, hesitancies, from which the scientific papers are considered excepted.

If Holton sustains that these elements are to be found within the private sources, my suggestion is that we should also be looking for them in the public ones, as it will be explained in the chapter on the heuristic value of continuity in Bohr's trilogy.

Holton distinguished three aspects as regards the thematic thought: 1) the *thematic concept*, e.g. symmetry and continuum. 2) The *methodological thema*, such as the preference to express the laws of science in terms, for example, of constancies. 3) The *thematic propositions*, viz. Newton's hypothesis concerning the immobility of the centre of the world.

[32] HOLTON, *Victory and Vexation in Science* (cit. note 4, ch. 1), p. 140.

According to Holton's account the "themata" is the historiographical key to understand the relations between science and humanities.

This "time-honoured" term is used in disciplines such as anthropology, art criticism and musicology. But Holton suggested that in scientific methods and hypotheses there are elements that constraint or motivate the individuals and sometimes they polarize the scientific community.

Thematic analysis, if adopted, could permit to answer to such questions:

«What is constant in the ever-shifting theory and practice of science - what makes it one continuing enterprise, despite the apparently radical changes of detail and focus of attention? What element remains valuable in theories long after they have been disproved? What are the sources of energy that keep certain scientific debates alive for decades? Why do scientists - and for that matter also historians, philosophers, and sociologists of science - with good access to the same information often come to hold so fundamentally different models of explanation? Why do some scientists hold on to models of explanation that are contrary to the evidence, sometimes at enormous risks?»[33].

If we pay attention to the method that Niels Bohr adopted for formulating the first atomic theory of 1912-13, we note that his atomic model was irrational from the point of view of the classical mechanics and electrodynamics. Bohr started from Rutherford's theory, according to which the atoms consist of a positively charged nucleus surrounded by a system of electrons kept together by attractive forces from the nucleus.

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[33] ID., "On the Role of Themata in Scientific Thought", *Science*, 1975, 188: 328-334, p. 329.

As we know, Rutherford assumed the existence of nuclei in order to account for the results of the experiments on large angle scattering of the α-rays[34]. He tried to explain some of the properties of matter on the basis of this atomic model, but the main difficulty was that of the instability of the system of electrons. Thus Bohr made use of a quantity foreign to the classical electrodynamics: Planck's constant, i.e. the elementary quantum of action, although he was not the first to introduce it. In fact, in atomic models, the quantization of energy via the use of Planck's constant was already introduced in 1910 by Arthur Erich Haas and in 1911 by John William Nicholson. Moreover, in 1905 Albert Einstein had already proposed the quantization of electromagnetic field. Nevertheless, Bohr distinguished himself for his insistence on a methodological consideration, which conceptual foundations seem to require an appropriate historical and philosophical study.

Bohr fully recognized the conceptual gap in his theory and he was convinced that the antithesis between quantum-theoretic and classical conceptions had to be brought to the forefront of theoretical analysis in order to achieve the progress in quantum theory[35].

It was in the attempt to trace the roots of this antithesis that he introduced the revolutionary conception of "stationary states". Bohr explained them as "some kind of waiting places between which occurs the emission of the energy corresponding to the various spectral lines"[36].

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[34] BOHR, "On the Constitution of Atoms and Molecules", *Philosophical Magazine and Journal of Science*, 1913, 26: 1-25, p. 1. Reprinted in LÉON ROSENFELD, HULRICH HOYER (eds), *Niels Bohr Collected Works* [NBCW], Vol. 2: Work on Atomic Physics, 1912-1917 (Amsterdam: North Holland Publishing Company, 1981), 161-185.

[35] JAMMER, *The Conceptual Development* (cit. note 3, ch. 1), p. 87.

[36] BOHR, "On the Spectrum of Hydrogen", in *The Theory of Spectra and Atomic Constitution* (Cambridge, GB: Cambridge University Press, 1922), 1-19, p. 11. Reprinted in ROSENFELD, HOYER (eds), *NBCW*, 2 (cit. note 34), 282-301, p. 293.

The idea of “waiting places”, in my view, plays a double role: on the one hand it refers to the electrons, on the other hand, it concerns the meta-level of his theory.

As a matter of fact, Bohr was aware that Planck’s idea of discontinuity was irreconcilable with classical physics. Therefore his “new” atomic model had to be conceived as a transitional phase towards a more coherent theory.

It was evident that Bohr regarded his theory as a hypothetical, incidental way of representing a number of experimental facts, which were inexplicable on the basis of classical physics.

It was an established fact that the ordinary electrodynamics and classical mechanics were unable to account for the stability of Rutherford’s atomic model and the way to follow was that of trying, firstly, an alternative, although approximate, solution; Secondly, of searching for harmony among the new ideas.

So far so good, but Holton himself wondered why some scientists hold on to models of explanation that are contrary to the evidence.

As I see it, Bohr’s methodological decision to persist in spite of the conceptual conflict is understandable in the light of the thematic concept of continuity between classical and quantum physics, which characterizes, as we have seen earlier, his research program.

I end this paragraph by stressing again Holton’s unsolved puzzle: what is the source of a person’s particular set of thematic concepts and hypotheses? Namely, what is the source of Niels Bohr’s thematic concept of continuity?

2.6 The Role of Philosophy

In *Victory and Vexation in Science* Holton concludes his reflections on the origin of themata with the hope that someday will be possible to approach them through studies of the nature of perception and apperception, i.e. the psychodynamics of the development of concepts in early life.

In 2001, three years before Holton completed his last work, Michael Friedman published *Dynamics of Reason*, which includes the well-known Kantian lectures held from the mid to late 1990s. Friedman's primary aim was to depict the deep and intricate connections between the historical development of scientific philosophy, on the one hand, and the parallel evolution of the exact sciences themselves, on the other. He started from the acknowledgement that Kant's original philosophical synthesis had failed due to unforeseen revolutionary changes within the sciences, and the logical empiricist's radical revision of this synthesis had also failed to do justice to the very rapid changes taking places within early twentieth century science. In response to this situation, Friedman offered the suggestion that we should attempt a fruitful interaction between contemporary philosophical practice and developments within the sciences themselves.

Furthermore, he suggested that distinctly philosophical reflection plays a special and characteristic function in transitions between radically different conceptual frameworks during scientific revolutions. At this point Friedman proposed to distinguish three different levels of discourse at work in such revolutions:

1. Properly empirical laws.
2. Constitutively a priori principles.

3. A meta-scientific level, where distinctively philosophical reflection takes place.

Friedman called the third level meta-paradigm or meta-framework. It plays an indispensable role in mediating the transmission of rationality across revolutionary paradigm shifts. In particular, when the concepts and principles of a later scientific research program develop through the concepts and principles of an earlier one, reflection on the distinctively philosophical level helps us to define such continuation. Therefore, we could add that philosophical reflection is the place where the themata come from. Namely, considering the transition from classical to quantum physics, we can suggest that the concept of continuity derives from such meta-scientific level.

Friedman gave the example of “natural philosophers” such as Descartes, Huygens, Leibniz who, in the seventeenth century, agreed on the first-level scientific paradigm of the mechanical philosophy. They had divergent standpoints on absolute versus relative motion. It was this disagreement, which, in part, alimented Newton’s own articulation of both a radically different first-level scientific paradigm (based on the laws of motion and the possibility of instantaneous action at a distance) and a radically different answer to the question of absolute versus relative motion.

It is worth noting that in this like in other case histories, Friedman aimed at showing how philosophical reflection interacts with properly scientific reflection in such a way that problematic philosophical themes become interwoven with relatively unproblematic scientific accomplishments.

As I see it, there is room to affirm that like in Niels Bohr’s case study, philosophical reflection can provide areas of scientific reflection with those concepts, which can facilitate the development of theories from their early stages. Friedman does not speak of thematic concepts, rather of properly philosophical themes, which are intertwined with scientific statements. As it shall be shown in the chapter to follow, it was reflection on philosophical level that historically produced Niles Bohr's themata of unity and continuity.

2.7 The Heuristic Role of Unity and Continuity in Bohr's First Atomic Theory

Lakatos identified the background problem of Bohr's program with the riddle of how Rutherford's atoms can remain stable, for, according to the Maxwell-Lorentz theory, they should collapse. According to Lakatos, Bohr's suggestion was to ignore for the time being the inconsistencies and consciously develop a research program whose refutable versions were inconsistent with the Maxwell-Lorentz theory. Thus Bohr proposed the two postulates [five according to Lakatos' reconstruction] as the hard core of his program.

The hard core would be the starting point of the new program, and the decision to not falsify the postulates would be the negative heuristic. The correspondence principle would stand for the positive heuristic. Lakatos stressed the crucial methodological difference between the inconsistency introduced in other scientists' programs and that introduced by Bohr. Bohr's research program contained no design planned in order to replace the older one (Maxwell-Lorentz theory). Even if it had been completely successful, would have left the inconsistency with the old theory unresolved. Bohr's idea to carry on his program in spite of the contradictions required great courage. As a Hevesy's letter testifies, the idea crossed Einstein's mind but he found it unacceptable, and rejected it.

The present paragraph is an attempt to understand what motivations do subtend such a decision. The answer, as I see it, must be found within Bohr's cultural and philosophical background, and in particular in certain concepts and schemes as parts of his cultural background.

The constant searching for harmony and unity that characterized Bohr's method of scientific inquiry needs something more than the appeal to the reason's natural tendency towards unity. Bohr's methodological decision arises in all its evidence from a passage of the speech delivered on December 1913 before the Danish Society of Physics, when he admitted that "Still the fact that we cannot immediately apply Planck's theory to our problem is not as serious as it might seem to be, for in assuming Planck's theory we have manifestly acknowledged the inadequacy of the ordinary electrodynamics and definitely parted with the coherent group of ideas on which the latter theory is based. In fact in taking such a step we cannot expect that all cases of disagreement between the theoretical conceptions hitherto employed and experiment will be removed by the use of Planck's assumption regarding the quantum of the energy momentarily present in an oscillating system. We stand here almost entirely on virgin ground, and upon introducing new assumptions *we need only take care not to get into contradiction with experiment*"[37].

Such a statement seems to be nothing but Bohr's aware decision to not contradict the new assumptions and to search for their harmonization in the "new mechanics", in the sense that what apparently seemed contradictory would have shown its coherence only later on. As I see it, it is such an intimate belief that asks for an explanation. With this in mind Bohr examined the experiments on temperature radiation as he focused on the

[37] BOHR, "On the Spectrum" (cit. note 36), p. 292. Italics are of the present author.

distribution of radiant energy over oscillations of the various wavelengths. Bohr assumed that the radiant energy comes from systems of oscillating particles. But, at the same time, he did not know anything about these systems, as no one had ever seen a Planck's resonator. It was only possible to observe the period of oscillation of the radiation, which was emitted. In order to obtain the laws of temperature radiation it was necessary to assume that the amount of energy emitted each time was equal to $h\nu$, where h is Planck's constant and ν is the frequency of the radiation. Thus Bohr showed that it was possible to bring this assumption about emission of radiation into agreement with the spectral laws:

«If the spectrum of some element contains a spectral line corresponding to the frequency ν it will be assumed that one of the atoms of the element (or some other elementary system) can emit an amount of energy $h\nu$. Denoting the energy of the atom before and after the emission of the radiation by E_1 and E_2 we have $h\nu = E_1 - E_2$ or $\nu = E_1 / h - E_2 / h$.

During the emission of the radiation the system may be regarded as passing from one state to another; in order to introduce a name for these states, we shall call them "stationary" states, simply indicating thereby that they form some kind of waiting places between which occurs the emission of the energy corresponding to the various spectral lines. As previously mentioned the spectrum of an element consists of a series of lines, whose wavelengths may be expressed by the formula [$1 / \lambda = F_r (n_1) - F_s (n_2)$]. By comparing this expression with the relation given above it is seen that – since $\nu = c / \lambda$, where c is the velocity of light - each of the spectral lines may be regarded as being emitted by the transition of a system between two stationary states in which the energy apart from an additive arbitrary constant is given by $-chF_r(n_1)$ and $-chF_s(n_2)$ respectively»[38].

By using this interpretation, the combination principle asserts that a series of stationary states exists for the given system, and that it can pass from one to any other of these

[38] *Ibidem*, p. 293.

states with the emission of a monochromatic radiation. Bohr demonstrated that with a simple extension of the first assumption it was possible to give a formal explanation of the most general law of line spectra.

In order to appreciate the importance of the so-called positive heuristic role of the concept of unity, which underlies Bohr's method, we would need to go back to the Manchester period, when Bohr took up for the first time the problem of the stability of Rutherford's atomic model.

We are taking in consideration the period May-July 1912, the same regarding the correspondence Bohr-Rubin. The electron theory of metals, which had brought Bohr to Cambridge, continued to be his primary commitment, that was stronger in the last days of May 1912 than it had been at any previous time during his stay in Manchester. Two months later, however, by his departure from Manchester to Copenhagen in late July, Bohr had shelved these problems indefinitely.

Indeed, in the early-mid summer of '12, Bohr turned from the electron theory of metals to the researches of Rutherford's laboratory. Bohr explained this transition to his brother Harald in a letter dated June 12, 1912:

«[…] I am not getting along badly at the moment; a couple of days ago I had a little idea with regard to understanding the absorption of α-rays (it happened in this way: a young mathematician here, C. G. Darwin (grandson of the real Darwin), has just published a theory about this problem, and I felt that it not only wasn't quite right mathematically (however, only slightly wrong) but very unsatisfactory in the basic conception), and I have worked out a little theory about it, which, even if it isn't much, perhaps may throw some light on certain things connected with the structure of atoms […]»[39].

[39] ROSENFELD, RUD NIELSEN (ed.), *NBCW*, 1 (cit. note 17, ch. 1), p. 555.

From the letter one apprehends that Bohr was unaware of Darwin's work until the time of its publication, therefore Bohr had for the first time engaged the local problem of α-absorption. Furthermore, he was pioneering the employment of Rutherford's planetary model in his own research. One week later, Bohr's scientific transition to the new researches became real conversion, as the enthusiastic letter he wrote his brother on June 19 testifies it:

«Perhaps I have found out a little about the structure of atoms. Don't talk about it to anybody, for otherwise I couldn't write to you about it as soon. I should be right it wouldn't be a suggestion of the nature of a possibility (i.e. impossibility, as J.J. Thomson's theory) but perhaps *a little bit of reality*. It has grown out of a little information I got from the absorption of α-rays (the little theory I wrote about last time)»[40].

What can Bohr have learnt from Darwin's paper by June 12? This crucial event reminds us of Lakatos' account about the formation of the program's hard core. Lakatos affirmed, with his picturesque typical words, that the hard core does not emerge fully armed like Athena from the head of Zeus but it develops slowly, by a long, preliminary, process of trials and errors. It means that the so-called hard-core's statements do not drop down from a superior mind, but they mature throughout technical and scientific problems, which the scientist has to deal with. The point is how to explain the subsequent decision necessary for not rejecting those statements. Lakatos, as I see it, failed to establish the foundation of such a decision, which, in the case of Bohr, is upheld by the concept of unity. The inter-translatability between metaphysics and heuristic, according to John Watkins, comes to assistance: «"all-and-some" doctrines (the concept of unity, for instance) may exhort the scientist to look for a certain kind of

[40] *Ibidem*, p. 559.

thing while appearing to inform him that that sort of thing is there to be found»[41]. Universal doctrines clash with certain kinds of falsifiable hypotheses and so prohibit their construction. Watkins termed such metaphysics as influential. In addition to Watkins' account I would highlight that inter-translatability between metaphysics and heuristic is also consistent with the formulation of the positive heuristic in a *Lakatosian* sense. Positive and negative heuristic, as regards their common metaphysical origin, would be different faces of the same medal. For this reason I assume that the principle of unity is also the hypostatization of Bohr's correspondence principle as far as "not a few doctrines, which are metaphysical, could be interpreted as typical hypostatization of methodological rules"[42]. As a matter of fact, in the practice of science certain heuristic rules are used, they guide the search for new knowledge by drastically restricting the range of possible decisions the scientist has to make in the searching process. Bohr tried to construct a new quantum theoretical explanation. In doing so he was guided by the idea that, for certain domains of phenomena, classical theory and quantum theory should lead to the same results.

As far as the epistemological analysis about the statute of the concept of unity is concerned, one could properly define this concept as a thematic one. According to Watkins' article, "Confirmable and Influential Metaphysics", in which he provided four degrees of empirical decidability as regard as the classification of the statements, one could classify the so-called "all and some" statements both as unverifiable and un-falsifiable, i.e. metaphysical. They coincide with scientist's thematic presuppositions, which do not arise from data or theory, but are imposed on them by him, as they

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[41] WATKINS, "Confirmable and" (cit. note 11), p. 356.

[42] POPPER, *Logik der Forschung* (cit. note 10), § 11 [1934].

constitute the mode of thought through which he understands the world. By the comparison of Bohr's quantum postulates with the classification proposed by Watkins, it follows that the former are second level statements: falsifiable but not verifiable. They are universal empirical hypotheses, which have been rendered metaphysical (in a *Lakatosian* sense) only by the decision to not falsify them for the time being. The decision is inspired by a fourth level, "all and some", statement, i.e. the thematic concept of unity. As well as the principle of correspondence is informed by the same thematic concept.

Now it is interesting to trace back to origin of Bohr's new research program.

Darwin's primary objective was to investigate Rutherford's model by applying it to the computation of the velocity loss of a α-particle moving through air or a thin sheet of metal. To make his computations manageable Darwin had introduced two related assumptions of which Bohr was extremely critical. The first was that a α-particle would not be impeded unless it actually penetrated the atom; the second that the intra-atomic forces on an electron could be neglected during the short time of its interaction with a rapidly moving α-particle. The latter assumption required the former, for, as Bohr pointed out in his critique, any computation which did include the interaction between atomic electrons and non-penetrating α-particles would yield an infinite result for the transferred energy unless the forces exerted by the rest of the atom on the electron were taken into account. Bohr rejected Darwin's first assumption at once. He agreed that the net force, outside the atom, on a passing particle due to the nucleus and electrons must be very near to zero, as far as the net force is relevant principally to scattering computations. Absorption computations demand the consideration of energy transfer; the nucleus, because of its weight, scarcely contributes; and the relevant forces are those between particle and electrons alone. Therefore, the force binding the electron into the atom must be taken into account. Bohr's first contribution to the problem was to recognize that the effect of this force depend critically on the relation between the period of the electron's motion and the collision time. It follows that knowledge of the frequencies of the electrons in an atom should permit a computation of absorption far more accurate than Darwin's[43]. But, unlike the Thomson's atom, in which the effective charge attracting an electron towards the atom's centre increases with the radius of the electron's orbit, the Rutherford's atom is mechanically unstable. Before Bohr took up the problem of the electronic structure, the Manchester group had been content to treat the electrons as uniformly distributed through the atomic sphere.

Bohr, however, could not long have remained unaware of mechanical instability. In fact, among *Bohr's scientific manuscripts*, the Niels Bohr Archive, are some forty sheets, which appear to record his first encounter with the problem of stability. They are collected in a file titled "Dispersion and Absorption of Alpha Rays" and they divide into three parts. The first two parts reach a conclusion, while the cover sheet of the third announces that it is "Temporarily Abandoned, since the Computation breaks down over the System's Instability, [and] cannot be continued without Applying some other Hypothesis". None of these sheets is internally dated. It is plausible to think that Bohr had completed the manuscript before June 12, the date of his letter to Harald about Darwin's calculations and Rutherford's atom. According to John Heilbron and Thomas Kuhn's account about the genesis of Bohr's atom, it is possible to conjecture that it was just this hypothesis that was discussed with Rutherford during the week prior to June

[43] Cf. JOHN L. HEILBRON, THOMAS S. KUHN, "The Genesis of the Bohr Atom", *Historical Studies in the Physical Sciences*, 1969, 1: 211-290, p. 239.

19. The hypothesis was: stabilization of the orbit by extra-mechanical fiat, through the introduction of Planck's quantum. As late as February 7, 1913, Bohr's research program remained that of the "Rutherford Memorandum". A month later, however, that program changed. Part I of Bohr's trilogy was sent to Rutherford on March 6, 1913. The subject was for Bohr entirely new: atomic spectra, in particular the line spectrum of hydrogen. In that interval of 9 months, Bohr got acquainted with a series of paper published by J. W. Nicholson, which dealt with the application of a quantized Saturnian model to the spectrum of the solar corona[44]. Because of his concern with Nicholson, Bohr became interested in the optical spectra for the first time during the early weeks of 1913. In any case, it was in such circumstance that H. M. Hansen, one of Bohr's assistance, told him about the Balmer's formula. So Bohr had to forge a quantum theory of atom mechanics and of radiation, which would permit the derivation of the Balmer's formula. Maxwell's theory demanded that a system of the sort considered by Bohr radiates energy and that the electron spirals rapidly into the nucleus. Bohr used the quantum to select and stabilize particular stationary states. Notwithstanding the contradictory procedure, Bohr was able to get around the obstacles of classical electrodynamics and to determine in quantitatively rigorous terms the permitted energy of any atomic element. But we cannot pass over the fact that there remained the problem on the internal consistency of a theory characterized by a logical incoherence that would discourage any other scientist. From this point of view, the new research program of quantum theory as a whole appears to us as a "grafted program" incompatible with the conservative

[44] As Bohr kept the distances from Nicholson's hypothesis of regarding the electrons disturbed during the process of recombination as responsible for the radiation emitted, he had to introduce a further assumption: $\nu = (1/2)\omega_n$. The last hypothesis is the first conceptual break in the framework of classical physics because it is the definite renounce to the existence of an immediate relation between optical and mechanical frequencies.

standpoints of some physicists. The conservative position would consider irrational to work on inconsistent foundations. Einstein took the decision to proceed despite temporarily inconsistent foundations in 1905, but he waved in 1913 whereas Bohr did not. Bohr's behaviour is an example of rational position as he exploited the heuristic power of his program without resigning himself to the initial inconsistencies on which the program was growing. Contrary to Planck and Einstein, Bohr did not try to bridge the abyss between classical and quantum physics, but he searched, from the very beginning of his work, for a system of quantum conceptions just as coherent as that of the classical notions. Bohr's plan was to work out the theory of the hydrogen atom. His model had to be based on a fixed proton-nucleus with an electron in a circular orbit. The idea that the atoms are analogous to planetary systems, in spite of the problem of the stability, presupposed a long and optimistic program. Bohr was convinced that the authentic key to the spectra had to be found, as if only time and patience would be needed to resolve his riddle completely. Therefore I suppose that such a conviction must have been inspired by a philosophical doctrine that he learned in the years of his youth, on which the concept of unity and the condition of continuity are hinged. Concretely speaking, the concept of unity did inspire his program's strategy, i.e. the correspondence principle. Bohr's paper of 1913 contained the initial step in his research program. It predicted facts hitherto unpredicted by any previous theory: the wavelengths of hydrogen's line emission spectrum. On the basis of his atomic model, Bohr was led to aspect that the "numerical values" of the classical frequency and of the quantum frequency would be approximately identical for a certain part of these spectra. With the aid of the correspondence principle it was possible to develop a complete quantum theory explanation of the Zeeman effect for the hydrogen lines, which in spite of the essentially different character of the assumptions that underlie the two theories, was very similar throughout to Lorentz's original explanation based on the classical theory. In the case of the Stark effect, where the classical theory was completely at loss, the quantum theory explanation was extended with the help of the correspondence principle as to account for the polarization of the different components into which the lines were split, and also for the characteristic intensity distribution exhibited by the components. However, although Bohr and others were able to explain many spectroscopic data on the basis of the principle of correspondence, they never made a decisive breakthrough. In fact, Bohr's theory of atomic structure was confronted with serious problems, such as that concerning the determination of the energy states of any atoms others then hydrogen atoms. In the end it was not Bohr himself but Werner Heisenberg who in 1925 articulated the foundation of a coherent quantum theory for which Bohr had been searching for so long. Nevertheless, Bohr saw Heisenberg's theory as "a precise formulation of the tendencies embodied in the correspondence principle"[45].

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[45] BOHR, "Atomic Theory and Mechanics", *Nature* (Suppl.), 1925, 116: 845-852; in *Atomic Theory* (cit. note 1, ch.1), 25-51, p. 49. Reprinted in *The Philosophical Writings of Niels Bohr*, Vol. 1: Atomic Theory and the Description of Nature (Woodbridge, CT: Ox Bow Press, 1987), 25-51.

Chapter 3

On the Historical Roots of Unity and Continuity

3.1 At the Origin of Thematic Analysis

Gerald Holton contributed to clarify Niels Bohr's cultural and philosophical background by presenting a brief but deep analysis of William James and Paul Martin Møller's thought as well as of Kierkegaard and Harald Høffding. He put emphasis on the gulf between the classical and quantum description, in particular between the essential continuity that underlies classical description, where coordinates such as space, time, energy and momentum can in principle be considered indefinitely indivisible, and the essential discontinuity and discreteness of the atomic processes. In the classical physics, causality was taken for granted, whereas in quantum physics the concept of indeterminacy, statistical description, and probabilistic distribution began to be accepted as an inherent aspect of natural description. Whereas in the classical physics a sharp subject-object separation was accepted, in the new physics it was seen that the subject-object coupling could be cut only in an arbitrary way. As Holton pointed out, Bohr's proposal of 1927 was to realize the complementarity of representations of events because in the ordinary language it is possible to explain the totality of nature only through a complementary mode of description[1]. For this reason – Holton noted – Bohr's preferred aphorism was Schiller's "Nur die Fülle führt zur Klarheit, / Und in Abgrund

[1] Cf. HOLTON, *Thematic Origins* (cit. note 20, ch. 1), pp. 101-102.

wohnt die Wahrheit" (Only the wholeness leads to clarity, / And the truth dwells in the abyss)[2].

Holton's reconstruction is the attempt to connect specific themes (viz. continuity, discontinuity, totality) to certain authors (both philosophers and scientists as well) as each theme was generated by a meta-historical context and the various authors were suspended from each theme respectively. Therefore, it might happen that different authors could share the same theme. Thus, the historian of science should try to ascertain whether any contacts between the authors were occurred. If the answer is in the affirmative, he should seek also to verify if in the course of time one author might have instilled the corresponding theme into the other. If the answer is in the negative, he rather should focus on the "genetic material" of the shared conceptions. I want to remark that Holton's work lacks of historical research as regards the philosophical debate at the basis of the thematic origin of scientific thought. Neither Holton nor Jammer stressed the methodological weight of the philosophical background for clarifying Bohr's decision to carry on the program notwithstanding the contradictions. In fact, both they suggested that was a peculiar philosophical background – although Holton stressed the themes, whereas Jammer emphasized the individual endeavours – to have influenced Bohr's scientific thought without saying anything about how it did it.

In my opinion, what both Holton and Jammer did was to point out some striking conceptual analogies among the avant-garde philosophers and some young physicists of the early twentieth century.

I would like to bring, in addition to the well-known Michael Friedman's reflections on the role of philosophy, William Whewell's teaching that, in some way, represents a

[2] WOLFGANG PAULI (ed.), *Niels Bohr and the Development of Physics* (London: Pergamon, 1955), p. 31.

forerunner in his attempt to shed light on the relation between science practice and history of science, as an example of historical reconstruction of the thematic origin of scientific thought. An issue of interest to philosophers of science is the relation between knowledge of the actual practice and history of science and writing a philosophy of science.

Whewell is interesting to examine in relation to this issue because he claimed to be inferring his philosophy of science from his study of the history and practice of science. His large-scale *History of the Inductive Sciences* (first edition published 1837) was a survey of science from ancient to modern times. He insisted upon completing this work before writing his *Philosophy of the Inductive Sciences, founded upon their history*. Moreover, Whewell sent proof sheets of the *History* to his many scientist-friends to ensure the accuracy of his accounts. Besides knowing about the history of science, Whewell had first-hand knowledge of scientific practice: he was actively involved in science in several important ways. In 1825 he travelled to Berlin and Vienna to study mineralogy and crystallography with Mohs and other acknowledged masters of the field. He published numerous papers in the field, as well as a monograph, and is still credited with making important contributions to giving a mathematical foundation to crystallography. He also made contributions to the science of tidal research, pushing for a large-scale world-wide project of tidal observations; he won a Royal Society gold medal for this accomplishment. Whewell acted as a terminological consultant for Faraday and other scientists, who wrote to him asking for new words. Whewell only provided terminology when he believed he was fully knowledgeable about the science involved.

In his section on the "Language of Science" in the *Philosophy*, Whewell made clear this position. Another interesting aspect of his intercourse with scientists becomes clear in reading his correspondence with them: namely, that Whewell constantly pushed Faraday, Forbes, Lubbock and others to perform certain experiments, make specific observations, and to try to connect their findings in ways of interest to Whewell.

In all these ways, Whewell indicated that he had a deep understanding of the activity of science[3].

The philosophical debate can generate concepts, ideas and themata with respect to science on a meta level as well as the history of science in Whewell's view produced knowledge and materials for science itself.

As it was said earlier, in Bohr's case we rather should focus on his insistence on continuity between the two physics that is exclusively a methodological issue. It's my claim that the concept of unity is deep-rooted in Danish culture as it originates in the reception of certain instances arising from the philosophical debate in the eighteenth and nineteenth century.

As it shall be shown, the philosophy of Harald Høffding – Niels Bohr's mentor – ascribed a fundamental importance to the couple unity/ continuity. For this reason I am going to present two hypotheses in order to explain the origin of such a thematic couple. One plausible hypothesis is that to refer such binomial to the so-called Leibniz-Wolffian system. The second hypothesis will deal with Post Kantian and German Idealism.

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[3] Cf. LAURA J. SNYDER, "William Whewell", *Stanford Encyclopedia of Philosophy*, 2006.

3.2 The Leibniz-Wolffian Monism

In the introduction of Johs Witt-Hansen's article, "Leibniz, Høffding, and the 'Ekliptica' Circle", from 1980 one can find an interesting reference to the Leibnizean "brand" of the Danish culture.

Witt-Hansen reminded that around the turn of the eighteenth century the reception of Leibniz' philosophy was in Germany still under the sway of Kant's critical analysis.

In the Appendix, "The amphiboly of concepts of reflection", contained in his *Critique of Pure Reason*, Kant attacked Christian Wolff's interpretation of Leibniz' metaphysical doctrine. Kant affirmed: «Leibniz intellectualised appearances, just as Lock sensualised all concepts of the understanding»[4].

He also explained that since Leibniz' doctrine is valid only for things in themselves, and not for appearances, it is not valid at all.

Schelling made a remarkable statement against Kant's critique of Leibniz' metaphysics in his *Abhandlungen zur Erläuterung des Idealismus der Wissenschaftslehre* (Essays in Explanation of the Idealism of the Doctrine of Science), written in 1796 and 1797:

«The history of philosophy provides examples of systems which have for ages remained obscure. A philosopher [Fichte] has recently passed the judgement upon Leibniz, that he was probably the only one in the history of philosophy to stick to his convictions, and, consequently, the only one who was basically right. This remark is noteworthy because it bears witness to the view that time has now come to

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[4] IMMANUEL KANT, "The Amphiboly of Concepts of Reflection", *Critique of Pure Reason* (1781, 1787). Translated into English by Norman Kempt Smith (London: St. Martin's, 1929), pp. 276-296. From JOHS WITT-HANSEN, "Leibniz, Høffding, and the 'Ekliptica' Circle", *Danish Yearbook of Philosophy*, 1980, 17: 31-58.

understand Leibniz. For if he is supposed to be basically right, he cannot be understood up to now. The subject deserves a closer investigation»[5].

Furthermore, Schelling continued his reflections on Leibniz in *Ideen zu einer Philosophie der Natur. Als Einleitung in das Studium dieser Wissenschaft* (*Ideas for a Philosophy of Nature: as the Introduction to the Study of this Science*):

«He [Leibniz] embodies the universal spirit of the world, which discloses itself in the most multifarious forms, and diffuses life where it happens to emerge. The claim that the right words for the presentation of his philosophy have not been discovered until now is therefore doubly insufferable; and it is likewise insufferable that the Kantian school obtrudes its feigned views upon him – charges him with statements on subjects where he in all cases precisely taught the opposite»[6].

Schelling's reflection allows us to give a hint of Leibniz' life and philosophy, as he grasped the essential unity that arises from the animate and rational nature. As I will show later on, Schelling joined Leibniz's view with Kant's dynamical theory of matter. In fact, Kant's transcendental philosophy was too great an obstacle to allow Leibniz's hylozoistic conception to supersede without a thorough critical analysis of the Kantian dichotomy between constitutive a priori principles and regulative a priori principles. Nevertheless, Kant was not the only responsible for the transitory misfortune of Leibniz' philosophy and scientific work. It was due more likely to the political situation in Hannover, ill disposed towards Leibniz' philosophical, scientific and technical

[5] FRIEDRICH W. J. SCHELLING, "Abhandlungen zur Erläuterung des Idealismus der Wissenschaftslehre" (1796, 1797), in *Werke* (München: Erster Hauptband, 1965), p. 367. From WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 31.

[6] ID., "Ideen zu einer Philosophie der Natur. Als Einleitung in das Studium dieser Wissenschaft" (1797, 1803), in *Werke* (cit. note 5), p. 670. From WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 32.

activities as Privy Court Counsellor to the Elector of Braunschweig-Hannover and (from 1714) to George I, king of England[7].

Leibniz died on November 14, 1716, and the entire court was invited to attend the funeral. No one attended except his secretary, Eckhart, and a close friend, the Scottish knight, John Ker of Kersland, who happened to be passing through Hannover at the time.

Leibniz was buried in an unmarked grave in the court church of Hannover. However, on each side of the coffin Eckhart placed 6 tin armorial ensigns or emblems. At the head of the coffin on the right side he placed the figure "1" circumscribed by a circle with the inscription *Omnia ad unum*, symbolizing the binary number system invented by Leibniz; and at the base on the left side he placed a spiral with the inscription *Inclinata resurget*[8].

As we have seen, Schelling tried to take up a fragment of Leibniz's thought by directing attention to the dynamic conception of nature that became an essential part of his own theory of nature.

In Denmark, Leibniz was rediscovered thanks to the Norwegian mineralogist and philosopher Henrik Steffens, who, as a follower of Schelling, gave Leibniz' theses a special relevance. In 1802-1803 he delivered at Ehlers Kollegium in Copenhagen the memorable "Introduction to Philosophical Lectures", which influenced Danish culture to such an extent that the debate about religion, philosophy, science and history in nineteenth century Denmark took a new direction.

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[7] Cf. WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 32.

[8] KURT MÜLLER, "Supra ossa Leibnitii", in "Leibniz. Der Internationale Leibniz-Kongress in Hannover", 1968, pp. 43-46.

Steffens became the spiritual author of Grundtvig's visionary conception of history, a conception nourished by "the idea of the association of ages and Christ in the midst of historical events"[9]. This historical vision inspired the Danish Folk High School movement, and found its expression in the fields of religion, poetry, history, mathematics, physics and technology at the Danish Folk High School in Askov in Southern Jutland.

To summarize Steffens' philosophical conception, it is worth noticing a brief report that he gave in a retrospect on his "Beyträge zur inner Naturgeschichte der Erde" from 1801, dedicated to Goethe:

«The subject which I tried to develop in this work became the main theme throughout my life, i.e. Existence as a whole should become history; I called it the intrinsic history of the Earth […]. Man himself should be regarded fully and entirely as a product of the development of nature […]. It became more and more clear to me that natural science itself, in so far as it introduced quite a new element in history, though which our time distinguished itself from the past as a whole, must become the most important of all sciences, the basis of the whole spiritual future of the human race»[10].

Moreover, in another text he added:

«Obviously it was my intention in writing this work to connect all phenomena of life in the unity of nature and history and, from the point of view of their unity, to trace the footprints of a divine purpose in the grandiose development of the universe»[11].

The above passages suggest a Steffens' dependence on Leibniz' universal conception of development, and a dependence on Leibniz' "Protogaea". After Steffens' attempt,

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9 NICOLAJ F. S. GRUNDTVIG, *Kort Begreb af Verdens Krønike i Sammenhæng* (Copenhagen: J.H. Schubothes, 1808), p. 26..

10 HENRICK STEFFENS, *Beyträge zur inner Naturgeschichte der Erde* (Freyberg: Erster Teil, 1801). From WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 34.

11 ID., *Was ich erlebte*. Aus der Erinnerung niedergeschrieben (Breslau: Vierter Band, 1841), pp. 286, 288-89. From WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 34.

Schelling's philosophical and scientific influence came to an end, although the Danish physicist and chemist Hans Christian Ørsted was able to transpose the idea of unity into physics. He proposed the unity of physical forces and the doctrine that "all phenomena are produced by the same original force"[12].

But, in Denmark, the principal mediator of Leibniz' philosophical thought was Gotthold Ephraim Lessing. In contradiction to Schelling, Lessing did not place emphasis on Leibniz' philosophy of nature, but he focused rather on Leibniz conception on religion and on his theory of truth and cognition, and did so in the light of Leibniz' basic philosophical tenet: the principle of continuity. Lessing presented Leibniz' conceptions as follows: whereas nature is the product of a continuous process of creation, religion is the process of a continuous revelation. Lessing's interpretation of Leibniz' thought was portrayed in a poetical form in "Eine Duplik" from 1778. It is worth reminding that Harald Høffding in his "History of Modern Philosophy" from 1885 quoted "Eine Duplik", adding that:

«The excellence mentioned by Lessing has a bearing on the expansion of man's powers in the act of research. However, such expansion would, none the less, be impossible or only a persistent disappointment if nothing whatsoever were achieved. Eternal striving without any achievements is in itself a contradiction in terms. It should not be overlooked, however, that any achievement is merely preliminary and becomes a starting point for a renewed striving. As will be shown below, this is precisely Lessing's conviction»[13].

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[12] HANS CHRISTIAN ØRSTED, "Thermo-Electricity", in *Naturvidenskabelige Skrifter* (Scientific Papers), (1820), later reprinted by Royal Danish Society of Science, 1928. From WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 35.

[13] HØFFDING, *Den Nyere Filosofis Historie* (cit. note 19, ch. 1), pp. 18-19. Translation from WITT-HANSEN, "Leibniz, Høffding" (cit. note 4), p. 36.

As Witt-Hansen noted, in the above passage is possible to discover the origin of Høffding's insistence on the non-finality of the process of cognition. As I see it, Witt-Hansen failed to recognize the most important evidence of Leibniz' methodological tenet with respect to Høffding's theory of knowledge: the principle of continuity that is inextricably associated with the idea of unity.

The principle of continuity is one of the most known concepts of Leibniz' doctrine. In a somewhat nebulous form this principle had been employed on occasion by a number of Leibniz' predecessors, including Cusanus and Kepler, but it was Leibniz who clarified it, and for this reason it was regarded as his own discovery. In a letter to Bayle of 1687, Leibniz gave the following formulation of the principle: "in any supposed transition, ending in any terminus, it is permissible to institute a general reasoning in which the final terminus may be included"[14]. This would seem to indicate that Leibniz considered "transitions" of any kind as continuous. Certainly he held this to be the case in geometry and for natural processes, where it appears as the principle *Natura non facit saltus*. According to Leibniz, it is the law of continuity that allows geometry and the evolving methods of the infinitesimal calculus to be applicable in physics[15]. In Leibniz' view, the principle of continuity is a necessary precondition for knowledge. To clarify, Leibniz deemed necessary that the principle of continuity would hold in the best of all possible worlds. In fact, Leibniz concluded his discussion of the principle of continuity with this remark:

«We see how the true physics should in fact be derived from the source of divine perfection».

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[14] CARL B. BOYER, *The History of Calculus and its Conceptual Development* (New York: Dover, 1959), p. 217.

[15] Cf. JOHN L. BELL, *The Continuous and the Infinitesimals in Mathematics and Philosophy* (Milano: Polimetrica, 2006), p. 93.

Leibniz employed the conservation principle and the principle of continuity as tests for the acceptability of proposed laws of impact and motion. The study of an ideal world, in which the law of order holds, would stand for a heuristic in the formulation of scientific hypotheses[16]. This is the reason why it does not contradict Leibniz' rejection of atomism doctrine.

In the "Demonstratio possibilitatis mysteriorum eucharestiae" from 1668, Leibniz claimed that the composition of the ideas of the things in god's mind does not give rise to "separate parts", but a re-composition of the varieties occurs in the mind's unity that is the fundamental concept of harmony, termed as *unitas plurimorum*, *unitas varietate compensata*, *diversitas unitate compensata*, *similitudo in dissimilibus*, *ratio identitatis*. The concept of unity/harmony is essential for understanding Leibniz's holistic philosophy and, as we shall see, is at the origin of Høffding's philosophical monism.

Harald Høffding coined the term critical monism to indicate in ontology the view of reality, which holds that it is one in number but that the unity embraces real multiplicity. Høffding gave the title of critical monism to the theory that reality, like conscious experience, is one although there are many items within that experience. Another example: both the one and the many exist and in the closest relation without either merging or cancelling the other. The one is immanent in the many although transcendent; the many are immanent in the one although in a sense beyond it.

Furthermore, it is worth noting that Leibniz expressed the concept of unity in his *Demonstratio* as follows:

«Unionis Mentis et Corporis sunt Ideae, uti Anguli uniones puncti cum lineis. Ideae sunt idem cum formis Substantialibus, Ideae ita sunt in Deo uti omnis action in agente, et uti Creatio est in Deo. Si quis quaerat:

[16] ROBERT C. SLEIGH JR., "Leibniz on the Two Great Principles of All Our Reasoning", in ROGER WOOLHOUSE (ed.), *W. G. Leibniz Critical Assessment* (London: Routledge, 1994), 31-58, p. 49.

an Idea sit creata an non? Respondere cogetur: creata sit creatura an non?»[17].

As we can read from the Latin quotation, the ideas are unifications of mind and body such as the angles unify points and lines. Leibniz anticipated with a geometric representation Høffding's critical monism. It's not my claim that Leibniz influenced directly Høffding, I rather sustain that Leibniz contributed to foster a cultural tradition that became predominant in Germanic world, and in nineteenth century Denmark. Perhaps Høffding took up the theme of Leibnizean monism as well as of Kantian Criticism, Idealism, Positivism and Existentialism, which he elaborated in a personal and original form. However, Leibniz's conception of unity and continuity alone wouldn't be sufficient to give account of the thematic concept of unity that I assume permeates Bohr's methodology of scientific research program, and in particular the complementary conception of subatomic phenomena if we are not able to shed light on Schelling's contribution to the revision of the fundamental distinctions, which Kant had introduced in the philosophy of nature.

3.3 Post-Kantian German idealism, *Naturphilosophie* and the Empirical Concept of Unity

Kant's philosophy of human knowledge and experience is based on a number of fundamental distinctions as it was also stated in the *Metaphysical Foundations of Natural Sciences* from 1786. As is known, Kant made a distinction between the passive or receptive faculty of pure intuition or sensibility (involving the pure forms of sensible intuition, space and time) and the active or intellectual faculty of pure understanding (involving the categories or pure concepts of the understanding: substance, causality,

[17] WILHELM GOTTFRIED LEIBNIZ, "Demonstratio possibilitatis mysteriorum eucharestiae", in *Sämtliche Schriften und Briefe* (Berlin: Akademie-Verlag, 1923), VI, 1, 501-17.

community, and so on). This distinction is at the basis of the dichotomy between appearances (spatio-temporal objects given to our sensibility) and things in them-selves (purely intellectual objects thought by the understanding alone).

The separation between constitutive a priori principles and merely regulative a priori principles is also related to such a distinction. Constitutive principles result from the application of purely intellectual representations to our spatio-temporal sensibility and yield necessary conditions for all objects of experience – conditions which therefore are necessarily realized in experience:

«[…] Hence, a complete analysis of the conception of matter in general must be laid at its foundation; this is the business of pure philosophy, which for the purpose makes use of no special experiences, but only of those which it meets with in separate (although in themselves empirical) conceptions, with reference to pure intuition is space and time (according to laws, essentially depending on the conception of nature in general), thus constituting it a real *metaphysic of corporeal nature*»[18].

The pure conceptions or categories of the understanding (i.e. substance, causality, and community) are necessarily realized in our experience by a system of causally interacting conserved entities distributed in space and time.

«But it is of the utmost importance in the progress of the sciences, to severe heterogeneous principles from one another, to bring each into a special system, so it may constitute a science of its own kind, and thereby to avoid the uncertainty springing from their confusion, owing to our not being able to distinguish of which of the two, on the one hand the limitations, and on the other the mistakes occurring in their use, are to be attributed. For this reason I have regarded it as necessary to present in one system the first principles of the pure portion of natural sciences (*physica generalis*) where mathematical constructions

[18] KANT, *Metaphysische Anfangsgründe der Naturwissenschaft* (1786). Translated into English by Ernest Belfort Bax, *Kant's Prolegomena and Metaphysical Foundations of Natural Science* (London: George Bell and Sons, 1883), p. 142.

traverse one another, and at the same time the principles of the construction of these conceptions»[19].

One of Kant's main purposes in the *Metaphysical Foundations of Natural Science* was to explain how the general constitutive principles of experience were specified to provide an a priori "metaphysical" foundation for the Newtonian model:

«But all true metaphysics is taken from the essential nature of the thinking faculty itself, and therefore in nowise invented, since it is not borrowed from experience, but contains the pure operations of thought, that is, conceptions and principles *à priori*, which the manifold of empirical presentations first of all brings into legitimate connection, by which it can become *empirical* KNOWLEDGE, i.e. experience. These mathematical physicists were quite unable to dispense with such metaphysical principles, and amongst them, not even with that which makes the conception of their own special subject, namely, matter, available *à priori*, in its application to external experience (as the conception of motion, of the filling of space, of inertia, etc.) […]»[20].

In the *Preface* to the *Metaphysical Foundations*, Kant denied scientific status to chemistry, as he asserted: "chemistry will be nothing more than a systematic art or experimental doctrine, but never science proper, its principles being merely empirical and not admitting of any presentation *à priori*"[21]. In Kant's view, chemistry and the more empirical sciences, except for Newtonian physics, are not provided with any metaphysical foundations. For this reason he invoked the famous doctrine of the regulative use of reason. The faculty of reason, unlike the faculty of understanding, generates a priori intellectual representations that cannot be fully realized in our spatio-temporal experience.

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[19] *Ibidem*, p. 143.

[20] *Ibid*.

[21] *Ibid*., p. 141.

«But besides the above internal necessity, whereby the metaphysical foundations of the doctrine of body are not only to be distinguished from physics, which employs empirical principles, but even from the rational premises of the latter, in which the employment of mathematics is to be met with, there is an external and, though only accidental, at the same time an important reason, for separating its thorough working-out from the general system of metaphysics, and for presenting it systematically as a special whole»[22].

As we have seen in the footnote 18, Kant put emphasis on the distinction between the pure concepts or categories and the principles of the construction of these conceptions. Here we can find one of the meanings as regards the concept of unity: "This separation besides the uses already mentioned, has the special charm, which the unity of knowledge brings with it, if we take care that the boundaries of the sciences do not run into one another, but occupy properly their subdivided fields"[23]. Nevertheless, this kind of unity does not pertain to the idea of unity that Kant mentioned amongst the ideas of the so-called regulative use of reason. These include the ideas of God, Freedom, and Immortality, and also the systematic unity of all empirical concepts and principles already generated by the understanding.

«For if it be permissible to indicate the boundaries of a science, not merely according to the construction of its object, and its specific kind of cognition, but also according to the aim that is kept in view as a further use of the science itself, and it is found that metaphysics has engaged so many heads, and will continue to engage them, *not in order to extend natural knowledge* (which could be done much more easily and certainly by observation, experiment and the application of mathematics to external

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[22] *Ibid.*, p. 147.

[23] *Ibid.*, p. 143.

phenomena), but *in order to attain to a knowledge of that which lies wholly beyond* all the boundaries of experience, of God, Freedom and Immortality […]»[24].

As Michael Friedman egregiously pointed out, this idea of systematic unity guides our process of inquiry in the more empirical and inductive sciences, without constitutively constraining it, as we successively ascend from lower level empirical concepts and principles toward higher-level concepts and principles. The goal of this process is an ideal complete empirical science of nature in which all empirical concepts and principles are constitutively grounded in the pure categories and principles of the understanding, but this is necessarily an ideal we can only successively approximate but never actually attain[25].

Post-Kantian German idealism – as successively articulated by Fichte, Schelling, and Hegel – is characterized by a rejection of Kantian dualism in all its implications. In particular, Kant's doctrine of the regulative use of reason is source of scepticism as regards most of the phenomena of nature, as only very few of these phenomena are constitutively grounded. As a consequence, it would appear that a great part of natural phenomena are not grounded at all.

For the idealists the conceptual structure of transcendental philosophy must be rejected, and with it the distinction between a passive faculty of pure sensibility and an intellectual faculty of pure understanding. However, as we have seen, Kant grasped the importance of the regulative use of reason under the idea of systematic unity, which guides our process of inquiry in the empirical sciences although it never fully attains its

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[24] *Ibid.*, p. 147.

[25] Cf. MICHAEL FRIEDMAN, "Kant – Naturphilosophie – Electromagnetism" in R. M. BRAIN, R. S. COHEN, O. KNUDSEN (eds.), *Hans Christian Ørsted and the Romantic Legacy in Science* (Dordrecht, NL: Springer, 2007), 135-158, p. 140.

goal. For Kant this process is both dialectical and infinitary, and above all is very natural once one has abandoned the dichotomy active intellect/ passive sensibility.

« […] In metaphysics the object is considered merely according to the universal laws of thought, […] but in other sciences it must be presented according to data of intuition (empirical as well as pure). Hence the former, because the objects must be invariably compared *with all* the necessary laws of thought must furnish a definite number of cognitions, which can be fully exhausted; but the latter because it offers an endless multiplicity of intuitions (pure or empirical), and therefore of objects of thought, can never attain to absolute completeness, but can be extended to infinity […]»[26].

Here arises the question of how such an infinitary dialectical process may be reconceived as an a priori constitution of nature. It is here that Schelling made his decisive contribution by sustaining the idea that matter, which appears to be dead and inert, is nothing more than an equilibrium of opposed forces. In his *Ideas for a Philosophy of Nature* from 1797, Schelling argued:

«In the dead object everything is at rest – there is in it no conflict, but eternal equilibrium. Where physical forces divide, living matter is gradually formed; in this struggle of divided forces the living continues and for that reason alone we regarded it as a visible analogue of the mind»[27].

Therefore, he opposed to the Newtonian conception of matter as made up of impenetrable, inert particles that are acted on by forces external to them. Then, he affirmed: "absolute […] inertness is a concept without significance"[28]. We have to remind ourselves that Schelling acknowledged that Kant's dynamical theory of matter, articulated in the *Metaphysical Foundations*, had already introduced an essentially

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[26] KANT, *Metaphysical Foundations* (cit. note 18), pp. 143-44.

[27] SCHELLING, "Ideen zu einer Philosophie der Natur. Als Einleitung in das Studium dieser Wissenschaft" (1797, 1803). Translated by E. Harris, P. Heath, *Ideas for a Philosophy of Nature*. (Cambridge, GB: Cambridge University Press, 1988), p. 177.

[28] *Ibidem*, p. 165.

dialectical element into nature. Indeed, in the *Explanation II* of the "Metaphysical Foundations of Dynamics", Kant stated:

«*Attractive force* is that moving force whereby a matter may be a cause of the approach of others to itself […]. *Repulsive force* is that whereby a matter can be the cause of repelling others from itself […]. The latter we shall also sometimes term driving, and the former drawing force»[29].

It means that the dynamical constitution of matter in general proceeds from the positive reality of expansive force (repulsion), through the negative reality of contractive force (attraction), to the limitation or balance of the two in the state of equilibrium. By uniting the concept of matter in general as conceived in Kant's original dynamical theory (the "dead" matter ruled by a dialectical process) with matter as conceived by *Naturphilosophie* – as an exhaustible source of rational life, Schelling overcame any possibility of a sceptical gap between our rational conception of nature and nature itself. What Schelling did was to fit Kant's conception of dynamical matter into the revised framework of the Fichtian idealism of his early works. With the term "revised" I mean that Schelling's dynamic conception of nature was no longer that of Fichte's dialectic of subject[30] and object, but of his own philosophy of the absolute. Schelling is here

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[29] KANT, *Metaphysical Foundations* (cit. note 16), p. 171.

[30] In his *Science of Knowledge*, Fichte took as his starting point the "perfectly certain and established" proposition A is A, or A = A, he then argued that this proposition requires a necessary connection (which he called X) between A as a subject and A as predicate. Fichte then argued that this connection must be present in the judging self, along with the A that forms the object and predicate of the judgement. Finally, just as Kant had argued that all connection of the manifold by the understanding rests on the original unity of the "I think" or consciousness, so Fichte argued that this connecting link (X) between the two sides of the judgement of identity also requires the unity of the self that makes the judgement: "He thereby arrives at the principles I am I, or I = I. The unity and identity of self-consciousness therefore form "the absolutely unconditioned principle of all human knowledge" as all knowledge must fall under the rule A = A, and this rule rests under the unity of consciousness which Fichte had deduced.
He then moved on to a second "perfectly certain and established" proposition: "not-A is not equal to A". This positing of not-A, Fichte argued, requires the original of A, and is therefore materially conditioned by this original positing. At the same time he argued that the act of counterpositing rests on the second principle of human knowledge, which states that a not-self must be opposed to the self; and just as the

returning to the Leibnizean "hylozoism" Kant explicitly rejected. In fact, this essentially biological or organic conception of nature then implies the overcoming of all scepticism in the sense that transcendental philosophy and *Naturphilosophie* – spirit and nature – are ultimately identical.

Schelling's endeavour is of great significance because is a way to show how philosophical speculation can interact with scientific discovery. As is known, Kant's concern was to provide chemistry and other natural sciences such as biology with a scientific status, which he would never achieve because of their theoretical apparatus[31]. However, Schelling rejected Kant's dualism, as *Naturphilosophie* focused on "the story of how nature itself successively unfolds or dialectically evolves from the "dead" or inert matter considered in statics and mechanics, to the essentially dynamical forms of interaction considered in chemistry, and finally to the living or organic matter considered in biology"[32]. But the key to Schelling's conception, as we have seen, is a

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not-A was conditioned by the original positing of the A, so that not-self must be conditioned by the original positing of the self.

Fichte next went on to argue that this counterpositing of the not-self by the self in fact leads into contradiction as it stands. For if the not-self is posited, the self is negated, but on the other hand, the not-self can only come to be if it is posited by the self; so if the former is to be posited, the latter cannot be negated. A way must therefore be found whereby the positing of the not-self does not absolutely negate and eliminate the self, and *viceversa*. The question therefore is "How can A and not-A, being and not-being, reality and negation, be thought together without mutual elimination and destruction?" The answer Fichte gave was that "they will mutually limit one another" where "to limit something is to abolish its reality, but not wholly, but in part only by negation". Thus, he agued, where the only way in which the contradictory nature of the second principle of human knowledge can be overcome is in the following third principle: "*In the self I oppose a divisible not-self to the divisible self*". In this way Fichte arrived at a pair of opposed moments that nonetheless require one another for their determination, and which can therefore be conjoined in a synthesis. Cf. Robert Stern's "Introduction" to *Ideas for a Philosophy of Nature* (cit. note 27), pp. XVII-XVIII.

[31] With respect to physics, Kant could deal with both empirical and a priori properties. For instance, the action of the universal attraction immediately exerted by each matter on all matters, and at all distances, that is called gravitation, is one of the most general properties of matter, therefore it deserves the metaphysical treatment to provide a priori insight into such properties. Vice versa, cohesion would not be a metaphysical but rather a physical concept, and so would not belong to this consideration. Plenty of similar examples could be provided by chemistry and biology of the time. Cf. FRIEDMAN, "Kant" (cit. note 25), p. 140, *ft*. 14.

[32] FRIEDMAN, "Kant" (cit. note 25), p. 143.

dialectical extension and elaboration of Kant's original dynamical theory of matter. Hence the new electrochemistry, which was developed in the early eighteenth century Denmark, was in Schelling's words the dialectical "middle term" between mechanism and biological (ultimately rational) living purposiveness.

«Therefore, already in the chemical properties of matter there actually lie the first, although still completely undeveloped seeds of a future system of nature, which can unfold into the most varied forms and structures, up to the point where creative nature appears to return back into itself. Thus, at the same time, further investigations are marked out, up to the point where the necessary and the contingent, the mechanical and the free, separate from one another. Chemical phenomena constitute the middle term between the two. It is this far, then, that the principles of attraction and repulsion actually lead, as soon as one considers them as principles of a *universal system of nature*»[33].

Indeed, as Andrew Wilson[34] explained, the *naturphilosophisch* vision contributed to foster the intellectual development of Hans Christian Ørsted, who enthusiastically embraced Schelling's key idea of chemistry as an extension of general dynamics. Following Schelling, Ørsted acknowledged the importance of the dialectical development of nature as a whole in order to realize its progressive unveiling of a single infinite rational life.

«A clearer perspective soon adds to this that there is nothing dead and rigid in nature, but that every thing exists only as a result of a development, that this development [*Entwicklung*] proceeds according to laws, and that, therefore, *the essence of every thing is based on the totality of the laws or on the unity of the laws*, i.e. the higher law by which it has been brought forth. Every thing, however, must again be regarded as an active organ of a more comprehensive whole, which again belongs to a higher whole so that only the great All sets the limit of this progression. And thus the universe itself would be regarded as the

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[33] SCHELLING, *Ideas* (cit. note 27), p. 149.

[34] ANDREW WILSON, "Introduction" to KAREN JELVED, ANDREW JACKSON, OLE KNUDSEN (eds.), *Selected Scientific Works of Hans Christian Ørsted* (Princeton, NJ: Princeton University Press, 1998).

totality [*Inbegriff*] of the developments, and its law would be the unity of all other laws. However, what finally gives the study of nature its highest meaning, *so they are in their application identical to thoughts*; the totality of the laws of a thing, regarded as its essence, is therefore an idea of nature, and the law or the essence of the universe is the totality of all ideas, identical with absolute reason. And so we see all of nature as the appearance [*Erscheinung*] of one infinite force and one infinite reason united, as the revelation of God»[35].

It is worth noting that Ørsted's vision originated from the new discoveries in electrochemistry, as interpreted, along with Schelling, as expressing the fundamental unity of magnetic, electrical, and galvanic forces.

I would like to point out that Schelling's primary teaching was to conceive nature as a dynamical evolutionary dialectical progression, so that transcendental philosophy and *Naturphilosophie* must ultimately coincide. There is a fundamental unity that appears to emerge from the nature outside us. It is this unity that guides the scientist in the process of inquiry in the empirical sciences.

In particular, there is room to sustain that Schelling's *Naturphilosophie* provided essential philosophical motivation for Ørsted's scientific discoveries. Nevertheless, it was objected that Schelling's speculative methodology is too abstruse to have exerted an influence on the cultural practice of science, and that Kant's original dynamical theory of matter, independently by Schelling, was sufficient to explain the further development of Ørsted's ideas.

But it is also beyond doubt that Ørsted carried on a metaphysical vision of rational mind and external nature as two complementary aspects of a single total organic development

[35] *Ibidem*, pp. 156-57. Italics are of the present author.

unknown to Kant. As he wrote in the "First Introduction to the General Doctrine of Nature [*Naturlaere*]" from 1811:

«In our knowledge of nature we distinguish between something which comes more immediately from reason and something else which rather has its origin in the senses. The two are in the most intimate connection with each other. It is the essence of man to present reason in an organic body, not merely in one particular form, but in its self-contemplating [*selvbeskuende*] totality. His sensuous nature, in the most exact sense, can only be regarded as the embodiment of this reason. Therefore, *the external sense organs already receive impressions in a manner which is in the most perfect harmony with it*, and an unconscious reason in the internal sense impresses in its own stamp even more markedly on these various abilities. Imperceptibly, *they thus approach the conscious reason which organizes and combines everything into even higher units* which, step by step, are finally transformed into the remarkable internal harmony of the independent reason. Thus, the science of experience (empirical science) comes into existence. *Reason, on its side, is similar to the internal foundation and essence of nature*. In a way, it contains the seeds of the entire world and must develop them through its necessary self-contemplation [*Selvbeskuelse*]. Consequently, it starts from the highest to which our spirit can ascend, from the essence of beings, the origin of everything. In itself, as a sign of this, *it seeks out the various primary directions and through them the origin of the essential fundamental forms in the eternal unity*. In its own laws it sees those of nature, in the variety of its own forms that of the world, and thus it develops and creates from itself All. In this way arises speculative natural science, which is also called philosophy of nature [*Naturphilosophien*]»[36].

Then Ørsted came to remark that "on the empirical path, we are stopped by the enormous profusion of objects which our senses offer, in which, however, there is no completeness"[37], and so he concluded:

«Speculative natural science seems to lead us more immediately to our goal, but here we would do well to bear in mind that the reason which reveals itself in nature is infinite while ours, which must discover it

[36] *Ibid*., pp. 288-89.

[37] *Ibid*.

here, is limited, trapped in finitudes. In innumerable spark, reason spreads through mankind. Although a reflection of the whole in every individual, it has in each its distinctive direction, which prevents it from spreading its light equally clearly and fully in all directions. Only recently shaped in its present form, speculative natural science will only approach significant perfection through the combined efforts of many thinkers»[38].

Ørsted saw, as Schelling did, nature as a realization of an infinite rational spirit, and regarded rational mentality as the evolutionary culmination of an infinite dialectical process by which sensible nature itself continually unfolds. For this reason there is, for Ørsted, a necessary harmony between speculative and experimental inquiry.

3.4 An Idealistic Interpretation of Niels Bohr's Rational Generalization Thesis

It is worth reminding that there is a trend in current Bohr scholarship that tried to make sense of Bohr's claim that classical concepts must be used for an unambiguous communication of experimental results in terms of a Kantian or neo-Kantian framework. But it's my aim to sustain an alternative approach.

A few years ago there was a scholar who wrote: "Bohr's understanding of physics must be related to some kind of metaphysical approach consistent with a Romantic philosophical outlook"[39]. Professor John Honner in his *The Description of Nature* stated that: "microphysics has became, in Bohr's work, a kind of metaphysics"[40]. Furthermore, Catherine Chevalley has argued that Bohr was much inspired by a range of Romantic philosophical writers, focusing on the meaning they ascribed to "symbols", "which

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[38] *Ibid*.

[39] STEEN BROCK, *Niels Bohr's Philosophy of Physics in the Light of the Helmholzian Tradition of Theoretical Physics* (Aarhus, DK: Institut for Filosofi Skriftserie [PhD Thesis] 1998), p. 175.

[40] JOHN HONNER, *The Description of Nature. Niels Bohr and the Philosophy of Quantum Physics* (Oxford, GB: Clarendon Press, 1987), p. 6.

could describe the special kind of knowledge which is not grounded in intuition (ordinary space and time)"[41]. Following these suggestions, I would like to express Bohr's doctrine of the indispensability of classical concepts in terms of his belief that quantum theory is a rational generalization of classical mechanics, which, in my view, is readable through the lens of the Romantic philosophical framework I have so far carried on.

The analogy between Bohr's doctrine of the indispensability of the classical concepts and the neo-Kantian interpretation is based on the consideration that, "the unambiguous interpretation of any measurement must be essentially framed in terms of the classical physical theories, and we may say that in this sense the language of Newton and Maxwell will remain the language of physicists for all time"[42]. But Bohr put forward also a second and more subtle meaning of the indispensability of the classical concepts:

«According to the view of the author, it would be a misconception to believe that the difficulties of the atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms […] No more is it likely that the fundamental concepts of the classical theories will ever become superfluous for the description of physical experience. The recognition of the indivisibility of the quantum of action, and the determination of its magnitude, not only depend on an analysis of measurements based on classical concepts, but *it continues to be the application of these concepts alone that makes it possible to relate the symbolism of the quantum theory to the data of experience*»[43].

It is hence understandable that classical concepts as position, momentum, force, etc. are essential for connecting the abstract symbolism of quantum theories to the experience.

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[41] CATHERINE CHEVALLEY, "Niels Bohr's Words and the Atlantis of Kantianism", in FAYE, HENRY J. FOLSE (eds.), *Niels Bohr and Contemporary Philosophy* (Dordrecht, NL: Kluwer Academic Publisher), p. 49.

[42] BOHR, "Maxwell and Modern Theoretical Physics", *Nature*, 1931, 128: 691-692, p. 692. Reprinted in KALCKAR (ed.), *NBCW,* 6 (cit. note 1, ch. 1), 359-60, p. 360.

[43] ID., "Introductory Survey" (cit. note 1, ch. 1), p. 16. The italics are of the present author.

To return to our analogy, could we claim that classical concepts are equivalent, in Kant's words, to the function of the passive or receptive faculty of pure intuition?

Before answering this question, I would like to point out that Bohr asserted that the discovery of the quantum of action brought about an entirely new situation with respect to human inquiry, as it introduced a necessary "individuality", "unity", "atomicity", "wholeness" or "indivisibility"[44], so that the quantum imposed a limiting condition upon classical descriptions. Honner synthesized the question as follows[45]:

1. The quantum of action is a discovery which is universal and elementary.
2. The quantum of action denotes a feature of indivisibility in atomic processes.
3. Ordinary or classical descriptions are only valid for macroscopic processes, where reference can be unambiguous.

In short, on the classical scale the effects of the quantum of action can be neglected and a provisional distinction between observer and observed can function adequately. Classical theory can thus be given as a "pictorial" interpretation in which theoretical and observational terms can univocally and unambiguously be applied, but such applications are invalid in the case of quantum physics, where a sharp distinction between observer and observed cannot be drawn.

However, Bohr's central idea that observation is possible thanks to the usage of classical concepts was motivated as follows:

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[44] Cf. KLAUS STOLZENBURG, *Die Entwicklung des Bohrschen Komplementaritätsgedankens in den Jahren 1924 bis 1929* (Stuttgart: PhD Thesis, 1977), p. 157. See HONNER, "The Trascendental Philosophy of Niels Bohr", *Studies in History and Philosophy of Science*, 1982, 13: 1-29, p. 10. Bohr uses the terms interchangeably. See also Bohr's draft "Mathematics and Natural Philosophy", where "wholeness" is replaced in turn of "indivisibility" and "unity"; Scientific Manuscripts: 20, p. 7 of each draft.

[45] Cf. HONNER, *The Description of Nature* (cit. note 38), pp. 82-83.

«To clarify the peculiar aspects of the observation problem in this field of experience […] it is decisive to recognize that, however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical term. The argument is simply that the word "experiment" we refer to a situation where we can tell others what we have done and what we have learnt and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics»[46].

In fact, we cannot delineate the "object" under investigation from the "measuring instrument" if object-instrument interaction is described quantum-mechanically. At the same time, we can communicate our experience of quantum events only in terms of their interaction with measuring apparatus. To this extent we must employ the concepts of classical physics, in which the idea of separability is explicitly involved. Moreover, this idea is the fundamental condition for observing the electron that can only be detected if it possesses an independent dynamic state in order to distinguish it from the state of the objects with which it interacts. This is to say that in order to obtain an unambiguous description of quantum events, we must refer every new concept to a sort of experiential framework expressible through the classical physics. We are again in sight of the question about the relationship between the reality and its "pictorial" representation.

Nevertheless, given that Bohr's stress was on the usage of concepts rather than on the status of the concepts themselves, he came to relate this usage to our participation in the world we experience. This seems to introduce a kind of holistic approach, as Honner

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[46] BOHR, "Discussion with Einstein on Epistemological Problems in Atomic Physics", in *Atomic Physics* (cit. note 43, ch. 1), 32-66, p. 39. Reprinted in *The Philosophical Writings of Niels Bohr*, 4 Vols., Vol. 2: Essays 1932-1957 on Atomic Phyics and Human Knowldge (Woodbridge, CT: Ox Bow Press, 1987), 32-66.

suggested in his *The Description of Nature*, for which questions about how language relates to the world cease to be relevant. But there is a further important step to take: from the unambiguous description to a complete and exhaustive description. How can we get a complete description of quantum events, if the observations concerning them cannot be combined in the usual way of classical physics?

Following Bohr's suggestions:

«The aspects of quantum phenomena revealed by experience obtained under such mutually exclusive conditions must thus be considered complementary in a quite novel way. The viewpoint of 'complementarity' does, indeed, in no way mean an arbitrary renunciation as regards the analysis of atomic phenomena, but is on the contrary the expression of a rational synthesis of the wealth of experience in this field, which exceeds the limits to which the applicability of the concepts of causality is naturally confined»[47].

Whereas the indispensability of the classical concepts could be accepted, the old cohesion guaranteed by the classical framework was no longer possible because the determinist union of causal and space-time accounts was broken. As a consequence, the possibility of a single picture universally covering all aspects of observation had to be abandoned[48]. In brief, quantum phenomena cannot be completely reduced in terms of classical concepts if we aim at obtaining an exhaustive description of reality, in as much as we have to do with the mutuality of observable and observer in the whole process of observation. In such an account truth is regarded as entailing "the asymptotic convergence of theory and reality"[49].

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[47] ID., "Biology and Atomic Physics", in *Atomic Physics* (cit note 43, ch. 1), 13-21, p. 19. Reprinted in *The Philosophical Writings*, 2 (cit. note 46), 13-21.

[48] Cf. HONNER, *The Description of Nature* (cit. note 40), p. 89.

[49] *Ibidem*, p. 17.

This asymptotic convergence between experience and theory is also consistent with the holistic account. Both the principle of correspondence and the concept of complementarity are involved in such a convergence, in as much as they are underwritten by Bohr's belief in the infinite and eternal harmony of nature.

«Likewise we must be prepared that evidence, obtained by different, mutually exclusive experimental arrangements, may exhibit unprecedented contrast and even at first sight appear contradictory. It is in this situation that the notion of complementarity is called for to provide a frame wide enough to embrace the account of *fundamental regularities of nature* which cannot be comprehended within a single picture»[50].

Thus, complementarity is the framework that makes it possible an exhaustive description of new experience. By using a slogan, we could say that complementarity is a kind of new causality. However, the concept of complementarity cannot be comparable to a new category of understanding, as it expresses "the synthesis of the wealth of experience", i.e. it has to do with phenomena considered in their own wholeness. Here once again rises the question of how such a wealth of experience may be grasped by "classical" categories. And here once again holists would argue for the possibility of scientific thought engaging a real world. This is to say that theory and reality are not entirely distinct, as they mutually shape each other, while they are also asymptotically convergent. At the same time, we have to point out that Bohr wrote about "the never-ending struggle for harmony between content and form" and he added that: "no content can be grasped without a formal frame"[51]. Consequently, as Honner noted, Bohr's holistic approach appears as moderate.

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[50] BOHR, "The Unity of Human Knowledge" (cit. note 24, ch. 1), p. 12. Italics of the present author.

[51] ID., "Discussion with Einstein" (cit. note 46), p. 65.

Bohr's aim was the reconciliation of classical and quantum theories into a rational and consistent whole, which his insistence on the wholeness of quantum-theoretical state-descriptions can give account of. Such a conviction would be sufficient to explain why Bohr was critical of Kant's thesis of a priori knowledge in formulating his own philosophy of complementarity[52].

In a similar vein, Shimony wrote that Bohr did not share Kant's view "concerning the structure of human knowledge, like the possibility of synthetic *a priori* judgments"[53].

If in his "Introductory Survey" from 1929 Bohr referred to space and time as "forms of perception" or "forms of intuition"[54], which would suggest a link to Kant, on the contrary – as it was recently noted – this was a connection Bohr was never explicitly acknowledged[55]. Indeed, Bohr was more inclined to emphasize the point that his own view differed from Kant's[56]. Bohr's belief in the unity in nature, as I suggest, rather appears to approach a post-Kantian and idealistic conception of nature itself.

To summarize, Bohr started from a Kantian presupposition of comparing classical concepts to forms of intuition, but the conditions for an objective description of microcosm brought him to reject the strict Kantian dichotomy between nature and its representation.

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[52] Cf. FOLSE, "Kantian Aspects of Complementarity", *Kantian Studien*, 1978, 69: 58-66 and DAVID KAISER, "More Roots of Complementarity: Kantian Aspects and Influences", *Studies in the History and Philosophy of Science*, 1992, 23: 213-39, pp. 220-26.

[53] ABNER SHIMONY, "Reflections on the Philosophy of Bohr, Heisenberg and Schrödinger", in ROBERT S. COHEN, LARRY LAUDAN (eds.), *Physics, Philosophy and Psychoanalysis* (Dordrecht, NL: D. Riedel, 1983), 209-21, p. 210.

[54] BOHR, "Introductory Survey" (cit. note 1, ch. 1), p. 5.

[55] Cf. CHRISTIAN CAMILLERI, *Werner Heisenberg and the Interpretation of Quantum Mechanics* (Cambridge, Cambridge University Press, 2009), p.143.

[56] Cf. FOLSE, *The Philosophy of Niels Bohr: The Framework of Complementarity* (Amsterdam: Elsevier, 1985), pp. 216-19.

Bohr saw the correspondence principle as the tendency to utilize in the development of quantum theory every concept of classical theories in order to find a synthesis between the thesis of the quantum postulates and the antithesis of the "classical theories" in a way that recalls the idealistic conception of reality. And again, we do not have to regard Bohr's conception just as a Hegelian dialectical synthesis, but rather the acknowledgement of the progressive rational development of nature that constantly shows the harmony among its items and forces, and hence guarantees that the asymptotically convergence between theory and reality, language and world, classical and quantum physics moves towards the truth, if never arriving at.

So Bohr went on to remark: "we must continually count on the appearance of new facts the inclusion of which within the compass of our earlier experience may require a revision of our fundamental concepts"[57].

Nevertheless, the concept of complementarity only apparently recalls a new transcendental "tool of reason":

«[…] We must realize that the discovery of the quantum of action has thrown new light on the very foundation of the description of nature, and *revealed hitherto unnoticed presuppositions* to the rational use of the concepts on which the communication of experience rests»[58].

In fact, if it is true that Bohr evidenced the shape of new rational presuppositions in order to grasp the subatomic phenomena, it is also true that he maintained that

[57] BOHR, "The Quantum of Action and the Description of Nature" (cit. note 48, ch. 1), p. 97.

[58] ID., "Atoms and Human Knowledge", in *Atomic Physics* (cit. note 43, ch. 1), 83-93, p. 91. Reprinted in *The Philosophical Writings*, 2 (cit. note 46), 83-93. Italics of the present author.

complementarity is above all a condition exhibited by "atomic objects obtained by different experimental arrangements"[59].

At the same time, such a relationship, expressed by the totality of phenomena "exhausts the possible information about the objects"[60], as though it unfolded "unnoticed" or "disregarded"[61] presuppositions of the understanding. In this sense there is an asymptotic convergence between our categories of understanding and the observed subatomic reality to the extent that the Kantian distinction between appearances and things in them-selves becomes untenable.

Furthermore, it is also worth noticing the fundamental divergence between Bohr and Kant's conceptions presented in the *Metaphysical Foundations*. As we have seen, Kant introduced a further distinction between "constitutive *a priori* principles" and "regulative *a priori* principles" from which he discerned the idea of systematic unity that guides our process of inquiry in the more inductive and empirical sciences without constitutively constraining it. We are in sight, for Kant, of the empirical hypothesis of unity, i.e. the reason's empirical presupposition of unity. There must be a relation, according to Kant, between the subjective, methodological aims, and the objective features in order to apply logical principles to nature. For this reason, homogeneity is necessarily presupposed in the manifold of the possible experience.

On the contrary, for Bohr, there are *fundamental regularities of nature* that must be comprehended (communicated). Bohr seems to reverse the question: regularities and

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[59] ID., "Quantum Physics and Philosophy – Causality and Complementarity", in R. KLIBANSKY (ed.), *Contribution to Philosophy in the Mid-Century* (Florence: La Nuova Italia Editrice, 1958), in *Essays 1958-1962* (cit. note 24, ch. 1), 1-7. Reprinted in *The Philosophical Writings*, 3 (cit. note 24, ch. 1), 1-7, p. 4.

[60] *Ibidem*.

[61] BOHR, "Biology and Atomic Physics" (cit. note 47), pp. 18-19.

unity in nature are not presupposed, as they are just objectively given and they are also expressed in the necessary conditions for the possibility of (experiential) knowledge. They are objectively given as it is given the unity or wholeness, atomicity, etc. introduced by the quantum of action.

But, on the other hand, such a unity, as we have seen, is also the rationale for the acquisition of objective empirical knowledge. In my view, it is this fundamental divergence on the idea of unity that makes it the difference with respect to Kant. Moreover, unlike Kant, Bohr did not regard the categories of understanding as eternally immutable.

In his concept of complementarity, considered, so to speak, “in quite a novel way”, we find a “content/form” interaction that recalls Ørsted’s metaphysical vision of rational mind and external nature as two complementary aspects of a single organic development. Coherently, Bohr saw science as moving towards, if never arriving at, the infinite harmony of nature.

Chapter 4

Harmony, Unity and Continuity in Bohr's Philosophical Reflection

4.1 The "Signs" of Bohr's Philosophical Inclination

As is known, classical mechanics presents an objective description of the reality, as it is based on the use of ideas referring to the events of daily life. But the introduction of Planck's quantum of action revealed in atomic processes a feature of wholeness foreign to the mechanical conception of nature.

Therefore, it became clear that the theories of classical physics are idealizations valid only in the description of phenomena sufficiently large to allow the neglect of the quantum of action.

On these premises the debate on the rational generalization thesis became central in Bohr's speculation, but more shall be said about this in the chapters to follow. Now I would like to return to Bohr's insistence both in the eternal harmony of nature and the unity of human knowledge, which, as we have seen, brings evidence to his holistic outlook.

Bohr was used to say that he was interested more in philosophy than in philosophers[1]. In other words, he was attracted by some philosophical themes in the history of

[1] As we will see, Bohr passed the general course in philosophy at the first year of the University with Harald Høffding. But beyond the relations with the Danish philosopher and his mentor, Bohr showed great interest in many philosophical discussions. In 1926 the "Selskabet for Filosofi og Psykologi" (the Society of Philosophy and Psychology) was founded in Copenhagen on Edgar Rubin's initiative. Here is a list of Bohr's participation in the meetings in this society that Professor Favrholdt has enumerated in his *Niels Bohr* (cit. note 21, ch. 1), pp. 33-34.

1) On December 7, 1928, he attended a lecture by Jørgen Jørgensen (Professor of Philosophy) on "The Geometrical Proof with Special Reference to Kant's Theory" and took part in the discussion afterwards.
2) On November 28, 1929, he himself gave a lecture: "Some Remarks concerning the Attitude to the Law of Causality in Recent Physics".
3) On March 27, 1930, he attended a lecture by Poul Tuxen (Professor of Indian Philology) on "Indian Considerations concerning the Validity or Non-validity of Knowledge" and again took part in the discussion after the lecture.
4) On November 4, 1932, he attended Rudolf Carnap's lecture: "Über den Charakter der philosophischen Probleme" (On the Character of the Philosophical Problems) and this time too participated in the discussion.
5) From February 16, 1933 to March 28, 1936, there were lectures by, among others, Louis Hjelmslev, Edgar Rubin, Wolfgang Köhler, Otto Neurath (twice), Viggo Brøndal, Bertrand Russel, and Ernst Cassirer. Unfortunately, the secretary of the Society wrote only the date, the name, and the title of the lecture, therefore we do not know who was present and who took part in the discussions.
6) On January 29, 1940, the members of the Society were invited by "Fysisk Forening" (The Physical Society) to join a meeting where Niels Bohr lectured on "Analysis and Synthesis in Past and Recent Physics".

scientific thought, which we can regard as recurrent in his life and works. According to Heisenberg, "Bohr was primarily a philosopher, not a physicist"[2]. The validity of Heisenberg's assessment is not given by Bohr's identification with a specific philosophical doctrine but rather by a fundamental concern with the conditions for the possibility of experiential knowledge.

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7) On November 24, 1941, Bohr attended a lecture in the Society of Philosophy and Psychology by Edgar Rubin, "On Wonder", and took part in the discussion after the lecture.
8) On March 11, 1943, the Society held a meeting on the occasion of Høffding's centennial. Frithiof Brandt (Professor of Philosophy) spoke on "The Discussion about the Philosophy of Life 1920-30' and at the same meeting Niels Bohr read out Høffding's Autobiography.
9) On May 31, 1946, Bohr was present and took part in the discussion after André Mercier's lecture: "Natural Laws and our own Theoretical Constructions".
10) On November 8, 1951, the Society celebrated its 25 anniversary. Niels Bohr was appointed honorary member together with the psychologist Albert Michotte and David Katz and gave a lecture on "The Use of Words".
11) In 1953 Gilbert Ryle gave a lecture on "The Concept of Mind", but the minutes have no date for the meeting. However, Professor Favrholdt participated in the conference and reminded that Niels Bohr attended the meeting and took part in the discussion.
12) On May 3, 1956, André Mercier gave a lecture on "Science and Philosophy" (in Danish). Bohr was present and here too took the floor in the discussion after the lecture.
13) On October 23, 1958, Bohr joined a meeting where Bent Schultzer (Professor of Philosophy) gave a lecture on "Relativity in a Philosophical Perspective". Again Bohr took part in the discussion with great eagerness.

[2] WERNER HEISENBERG, "Quantum Theory and its Interpretation", in STEFAN ROZENTAL (ed.), *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (Amsterdam: North-Holland, 1967), p. 98.

Niels Blædel in *Harmony and Unity. The Life of Niels Bohr*[3] gave an original account of Bohr's philosophical ideas. Blædel wrote that Bohr's main philosophical idea arose for the first time when he saw that Rutherford's atomic model "could not be married with classical physics". As he realized the inevitability of the break, he had the courage to carry it through. He also had "the intuition to make successful guesses" as to achieve further progress in the course of time, and – Blædel continued – "his guesses sprang from his insight into the germ of the paradox in nature herself".

Blædel saw the following quotation from "The Unity of Human Knowledge" as indicative of Bohr's "courage", i.e. the human means of expression allow the great variety of subject matters to find its unity.

«The aim of our argumentation is to emphasize that all experience, whether in science, philosophy or art, which may be helpful to mankind, must be capable of being communicated by human means of expression, and it is on this basis that we shall approach the question of unity of knowledge»[4].

Bohr's approach could be seen as the attempt of understanding the unity of human knowledge from the unity of the phenomena of nature, which can be "communicated" through the human means of expression. This seems to refer again to the necessary condition for an objective description of all experience.

Both Abraham Pais[5] and Blædel have related the following episode that happened during the summer of 1942 in the pavilion of Tisvilde, a small town located on the north coast of the island Zealand in Denmark. Bohr was preparing the address he was to give on the occasion of the tercentenary celebration of Newton's birth. He stood in front of

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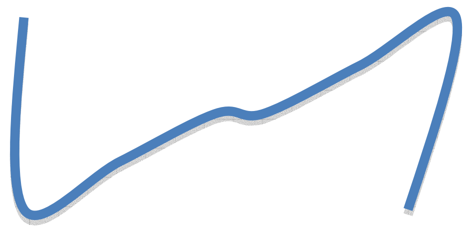
[3] NIELS BLÆDEL, *Harmoni og enhed* (Copenhagen: Carlsbergfondet, 1985). English translation: *Harmony and Unity* (Madison, WI: Science & Tech. Inc., 1988), pp. 186-87.

[4] BOHR, "The Unity of Human Knowledge" (cit. note 24, ch. 1) , pp. 14-15.

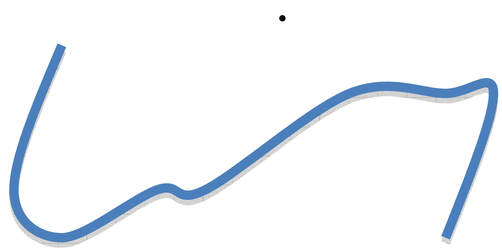
[5] ABRAHAM PAIS, *Niels Bohr's Time. In Physics, Philosophy, and Polity* (Oxford: Clarendon Press, 1991) p. 10.

the blackboard and wrote down some general themes to be discussed. One of them dealt with the harmony. So Bohr wrote down the word harmony.

It looked about as follows:



As the discussion progressed, Bohr became dissatisfied with the use of harmony. He walked back and forth restlessly, then he suddenly stopped and his face lit up. According to Blædel, Bohr exclaimed: "Now I've got it. We must change harmony to unity"[6]. So he picked up the chalked again, stood there looking for a moment at what he had written before, and made the single change:



With one triumphant bang of the chalk on the blackboard. Even though Pais did not pay too much attention to the episode, it is sufficient to guess that the theme of unity was current in Bohr's mind and preceded any intention to popularize also the most mysterious concepts of quantum physics.

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[6] It is worth noticing that in his *Niels Bohr's Time*, Pais reported: "Now I've got it. We must change harmony to uniformity". But Blædel has confirmed his version.

As we know from his philosophical papers, Bohr sought to develop a general philosophical point of view, which would revolutionize our understanding of how human knowledge relates to the world, which forms its object. He also held that this general philosophical outlook taught an epistemological lesson. Nonetheless, the relatively small stack of essays prepared for specific occasions, such as addresses to various professional audiences, is characterized by the lack of detail and general obscurity of the argument behind many of Bohr's key claim.

As Jan Faye and Henry J. Folse noted[7], partially for this reason, and partially because of the plenitude of various "interpretations" of quantum theory, scholars remain deeply divided over just what Bohr's message is. Professor Sandro Petruccioli[8] wrote that Bohr's essays often led up to complementarity by way of an historical survey of the development of quantum physics.

Petruccioli emphasized what he called Bohr's rational reconstruction of the story, in which events are selected with the very definite philosophical objective of making the complementarity viewpoint appear as inevitable and most satisfying completion to this development.

Ulrich Röseberg[9] spoke of "hidden historicity" to show that Bohr's perception of these events shaped his own personal reasoning towards complementarity. In my opinion, the above concept of unity is the leitmotiv underlying Bohr's philosophical and scientific thought. It is this concept that shapes Bohr's personal reasoning giving to it coherence and linearity in the context of a general view of the world.

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[7] Cf. FAYE, FOLSE (eds.) *The Philosophical Writings of Niels Bohr,* Vol. 4: Causality and Complementarity (Woodbridge, CT: Ox Bow Press, 1998), p. 1.

[8] PETRUCCIOLI *Atoms, Metaphors* (cit. note 6, ch. 1), pp. 134-172.

[9] ULRICH RÖSEBERG, "Hidden Historicity: The Challange of Bohr's Philosophical Thought", in FAYE, FOLSE (eds.) *Niels Bohr and Contemporary Philosophy* (cit. note 41, ch. 3), p. 331.

In the first of the two talks given on the Danish Radio, April 1963, Aage Petersen said: "Bohr's philosophy is not built around a principle or a doctrine, as its core is not a thesis, but a viewpoint or an attitude, and it is difficult to illustrate this attitude by examples for daily life"[10].

Bohr's inclination towards the concept of unity rises explicitly in the speech addressed to the International Congress of Pharmaceutical Sciences, "The Connection between the Sciences", from 1960. In particular, here we find one of the most important definitions of unity:

«In this address I have tried to show how researches into the world of atoms have offered new opportunities of tracing that harmony in nature of which Ørsted spoke, but which we perhaps would rather refer to as the unity of human knowledge. It is indeed only the appreciation of such harmony or unity which can help us to keep a balanced attitude to our position and avoid that confusion which the tumultuous progress of science and technology in almost every field of human interest may so easily produce»[11].

Bohr started from the elucidation of Hans C. Ørsted's concept of unity, which was regarded as the unification of all physical forces into nature, and then he referred the concept of unity to human knowledge. Nevertheless, Bohr and Ørsted's standpoints are compatible, as they consider rational mind and external nature as two aspects of a single organic development. In fact, both they share the broader philosophical vision of a

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[10] AAGE PETERSEN, "Niels Bohr's Philosophy", Two talks given on the Danish Radio on April 1963. The transcription of the interview is available at The Niels Bohr Archive. The interview was later published in Petersen's article "The Philosophy of Niels Bohr", *Bulletin of the Atomic Scientists*, 1963, 19: 8-14. But the above quoted passage has been omitted.

[11] BOHR, "The Connection between the Sciences", in *Journal Mondial de Pharmacie*, 3, Juillet-Decembre 1960, 262-67, in *Essays 1958-1962* (cit. note 24, ch. 1), 17-22. Reprinted in *The Philosophical Writings*, 3 (cit. note 24, ch. 1), 17-22, p. 22.

dialectical or evolutionary development of nature in the realization or progressive unfolding of a unified and harmonic rational life.

At the same time, researches in modern atomic physics have shown and confirmed the existence of harmony and regularities in nature, to which the unity of knowledge cannot be completely reduced. Bohr appears to hint at the interaction nature-reason and observable-observer in the attempt to point out the difference between the holistic outlook and the reductivist aims of the positivists.

However, Bohr's unspoken aim was the heuristic function of the concept of unity that also played an essential role in the formulation of the first atomic theory.

We should hence consider three levels of analysis as regards the concept of unity: the asymptotic unity among the sciences, asymptotic unity among science and humanities, and asymptotic unity between classical and quantum physics. Here one can identify the heuristic function of such a concept.

The "balance attitude" which Bohr speaks of is the harmony towards which science's progress tends, without ever arriving at, as it helps us in superseding the initial incompatibilities among the various scientific and technical disciplines. Such a concept is consistent with the definition of continuity theorist, and goes far beyond the apparent encyclopaedic meaning that a literal interpretation folds.

Furthermore, if we look through the previously quoted address, "The Unity of Human Knowledge", we can find also the most evident connection between the concept of unity and that of complementarity in the specific context of the rational generalization thesis that can give significance to the above-mentioned Bohr's "courage":

«A detailed account on complementarity lines of a new wide domain of experience has been possible by the gradual establishment of a mathematical formalism, known as quantum mechanics […]. Just by treating the quantum of action as an element evading customary explanation – similar to the role of the velocity of light in relativity regarded as a maximal speed of signals – this formalism can be regarded as a rational generalization of the conceptual framework of classical physics»[12].

Bohr went on in emphasizing the considerations as regards the features of wholeness consistent with such a position. In the next paragraphs Bohr mentioned again Poul Martin Møller's novel in order to illustrate the issue of both the arbitrary separation of the subject-object coupling with respect to quantum physics, and the separation between the content of our consciousness and the background loosely referred to as "ourselves" with respect to psychology. Bohr came to introduce a new mode of reasoning that allows us understanding how poetry, painting and music may "contain possibilities of bridging between extreme modes as those characterized as pragmatic and mystic". And again:

«Conversely, already ancient Indian thinkers understood the logical difficulties in giving exhaustive expression for such wholeness. In particular, *they found escape from apparent disharmonies in life by stressing the futility of demanding an answer to the question of the meaning of existence*, realizing that *any use of the word "meaning" implies comparison*; and with what can we compare the whole existence?»[13].

Bohr was aware of this new kind of "logic" that he had already found in the reading of the Chinese Tao-te-Ching of Lao-Tse since he was a youngster[14]. Such as the apparent disharmonies in life could be avoided if we took on a holistic approach, it seems

[12] ID., "The Unity of Human Knowledge" (cit. note 24, ch. 1), pp. 12-13.

[13] *Ibidem*, p. 14. Italics are of the present author.

[14] In a letter to a teacher, Svend Hugo Jürgensen, date March 26, 1958, Bohr wrote: «Thank you for your letter and the enclosed leaflet on Tao Teh King, which I read with interest. The things you say about ancient Chinese philosophy are, I believe, in many ways to the point. In my youth I received a beautiful impression of it through Ernst Møller's book "Oldmester", and during a visit to China twenty years ago I learnt how much the memory of Lao-Tse still is appreciated». From FAVRHOLDT, *Niels Bohr* (cit. note 21, ch. 1), p. 37.

reasonable to hold that the apparent disharmonies in the sciences may be superseded by adopting a similar outlook. Moreover, it should not prevent to affirm that Bohr had the courage to carry through the quantized Rutherford's model in spite of the apparent contradictions because of his characteristic standpoint. I want to highlight that Bohr's stress on the rational generalization thesis pertains both to the concept of complementarity and the formulation of the first atomic theory. Whereas it is true that Bohr's enunciation of such a thesis was made only later on[15], it is also true that such a conception was applied since the early years of his research, as it shall be shown in the next chapters. This is to say that Bohr's scientific and philosophical thought was characterized since the outset of his research activity by a general conception of the world that historians of science have called with various names: wholeness, harmony, homogeneity, totality, uniformity, but, as I see it, they are expression of the condition of unity and continuity.

4.2 Bohr and Logical Positivism

Bohr's connection to the logical positivism was seen as an important evidence of his general interest in philosophy. In particular, the concept of unity of knowledge allows understanding the fundamental difference between Bohr and neo-positivists. As is well known, Bohr took part in the "2. *International Kongress für Einheit des Wissenschaft*" held in Copenhagen from June 21 to 26, 1936. Otto Neurath and Jørgen Jørgensen

[15] The first occurrence of Bohr's formulation of the rational generalization thesis goes back to the "Introductory Survey" from 1929, in *Atomic Theory* (cit. note 1, ch.1), 1-24, p. 4. Reprinted in KALCKAR (ed.), *NBCW*, 6 (cit. note 1, ch. 1), pp. 279-302: «The endeavours to formulate general laws for these possibilities and probabilities by a suitability limited application of the concepts of the classical theories have led recently, after a series of phases in its development, to the creation of a rational quantum mechanics by means of which we are able to describe a very wide range of experience, and which may be regarded in every respect as a generalization of the classical physical theories».

organized the conference. They were the two among the positivists, together with Philipp Frank, with whom Bohr had closest contact[16].

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[16] At The Niels Bohr Archive, included in *Bohr Scientific Correspondence*, is available the plentiful correspondence between Philippe Frank and Niels Bohr from 1935 to 1962.

- In the summer of 1935 (?) Frank in Prague writes to Bohr in Copenhagen: he thanks him for the letters to the Russian colleagues and has at once sent them off. He expects with great interest Prof. Bohr's report of his lecture in Copenhagen. He thinks it should be very profitable once to speak quietly about all these questions. He has the impression that they have still more importance for the ordinary understanding of science, but there are still many misunderstandings.
- 5 July 1935 Bohr in Copenhagen writes to Frank in Prague: Prof. Bohr writes that professor Jørgensen some weeks ago has been so kind to show him the article which Frank has written on the relations between atomic physics and the ordinary problems of biology and psychology. Although Prof. Bohr has been pleased of Frank's humour, and although he in something agrees with Frank, he must confess that on the tendency his endeavours are not quite in agreement. Regarding the discussion of the real physical problems has Frank perhaps in the meantime read the last article of Einstein and his collaborators, to which Prof. Bohr has just written an answer, of which he encloses a copy, and hopes that Frank, although he is not quite satisfied with the mode of expression, will content to it.
- 9 January 1936 Frank in Prague writes to Bohr: Prof. Frank has lately occupied himself in detail with the article of Prof. Bohr in the Physical Review "Vollständigkeit der quantenmechanischen Beschreibung", and has given two lectures about the article of Einstein and that of Bohr. He writes quite a lot about this and other scientific questions.
- 14 January 1936 Bohr writes to Frank: Prof. Bohr thanks for Frank's two letters and was pleased to hear that Frank has occupied himself so thoroughly with works of Einstein and himself, and he thinks that Frank has quite understood his endeavours. Bohr writes a long letter about the scientific questions. He looks forward to see Prof. Frank in Copenhagen this summer and will speak with Jørgensen about the questions in Frank's letter concerning the congress. Bohr has already written to Neurath about it. The named article of Jordan Bohr has not seen at all.
- 1936 Frank in Prague writes to Bohr: Prof. Frank thanks for the kind interest which Prof. Bohr has shown in his brochure, which Prof. Jørgensen has shown him. Frank is quite convinced that they will agree in all the questions when they some time have the opportunity to speak thoroughly about them. Writes about scientific questions.
- 27 May 1936 Bohr writes to Frank: Prof. Bohr is sorry that he has not been able to send the promised draft to his lecture at the Copenhagen congress about causality. He has been occupied with urgent duties and was thereafter somewhat overworked, so that he has not come rather far with lecture. He writes about the scientific questions, but hopes after Whitsuntide to be able to summarize his lecture and to send it to Frank.
- 21 December 1936 Bohr to Frank: Prof. Bohr sends the manuscript of the lecture he had given in the last summer at the Congress in Copenhagen. As Mr Neurath is in USA ate present Prof. Bohr also in three weeks goes to America, he sends the manuscript to Prof. Frank asking him to take care that it is given in print as soon as possible, so that Prof. Bohr could see a proof before his departure. Mr Neurath has proposed that Prof. Bohr's paper should be published as a special publication together with that of Prof. Frank and that of Schlick. But Prof. Bohr does not agree with his idea, but thinks it more reasonable that all the lectures are published in the "Erkenntnis".
- 26 January 1937 Niels Bohr's secretary to Frank: A secretary of Prof. Bohr thanks for Prof. Frank's letter which reached him before his departure, he was very happy that Prof. Frank agreed with him as regards the publication of the three letters. From the printers Meiner has come a proof, of which a copy, as promised is sent to Prof. Frank. Prof. Bohr has only made small alterations.

Bohr never proclaimed himself a "positivist", as it is deducible also from a letter quoted in the footnote [5th July 1935] that Bohr sent to Frank. Bohr showed his disagreement on the discussion of the physical problems with Frank, in particular on EPR's paradox. However, Bohr was invited to write an introductory statement titled "Analysis and Synthesis in Science" for the first volume of the *International Encyclopedia of Unified Science* edited by Neurath and Jørgensen, in addition to Carnap, and he served on the Advisory Committee of the project. A photograph, taken during the conference, shows many of the participants sitting in the hall of Carlsberg's honorary mansion where Niels Bohr at that time lived. Among the audience you find Otto Neurath (1882-1945), Carl Gustav Hempel (1905-1997) and Karl Popper (1902-1994), but also some of the more prominent Danish scientists and scholars whose world views were congenial with the logical positivists. In the foreground Jørgen Jørgensen (1894-1964) stands half turned towards the photographer, half turned towards the participants whom he is about to welcome. Jørgensen was the general secretary of the conference and had been, together

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- 4 June 1952 Frank in Boston writes to Bohr in Copenhagen: Frank recently studies with great interest Prof. Bohr's discussions with Einstein about the epistemological problems of Quantum Theory. Frank is just writing a textbook on the philosophy of science in which he should like to discuss the foundations of quantum theory in a way which is a little mysterious as possible. Recently the Harvard University Press has planned to publish a series of books on the philosophy of the sciences each of which would treat a limited domain. Frank shall be editor of the series and the director of the Harvard University Press asked him to make a recommendation as to that books it would be wise to publish. He would like to ask Prof. Bohr whether he would be interested in the publication of a small book on the philosophical foundations of sub-atomic phenomena which would contain his views on the fundamental issues. If Bohr does not like to devote much time to it, Frank would try to find a scientist who could put Bohr's paper together with some connecting text by Frank himself, and Bohr would be asked only to look it over and indicate what corrections he would like to make.
- R.S. Cohen in Boston to Bohr (with one enclosure): "Since it is not likely that you can attend the dinner for Philipp Frank, may I suggest that a cablegram sent to him to arrive on May 25th at the Harvard University Faculty Club would give him great pleasure?
- 24 may 1962 Bohr to R.S. Cohen in Cambridge (MA.), telegram: "On the occasion of the 50th anniversary of Philipp Franks's appointment to the chair of theoretical physics at the Charles University I take great pleasure to join his many other friends in conveying our admiration for a rich life work devoted to the elucidation of the foundations of science and its place in contemporary culture.

with Neurath, the primary motor behind the organization of the meeting in Copenhagen. Behind Jørgensen, to the right on the first row of seats, is Niels Bohr (1885-1962) sitting next to Philipp Frank (1884-1966). Right behind Bohr is George de Hevesy (1885-1966), and again behind him, on the third row, you see Harald Bohr (1887-1951) professor of mathematics. On some of the other rows you find Alf Ross (1899-1962), a Danish philosopher of law, and Edgar Rubin (1886-1951).

Many chairs in the first two rows are empty. This may due to the fact that several of the invited guests had difficulties getting to the opening of the conference because of the political situation in Germany and Austria. Philosophers like Moritz Schlick (1882-1936), Rudolf Carnap (1891-1970), and Hans Reichenbach (1891-1953) had all expressed their wishes to be in Copenhagen, but various reasons prohibited them from coming. Thus, Schlick had been denied a travel permit from Austria which turned out to be fatal. A mentally deranged student killed him at June 22 at the steps of the University of Vienna. The conference in Copenhagen received the message about Schlick's death with horror. At that time Reichenbach was staying in Turkey as a refugee and could not afford the long journey to Denmark. And several of the most prominent members of the Circle had fled to America where Herbert Feigl (1902-1988) arrived in 1931 and Rudolf Carnap (1891-1970) in 1935 from Prague[17].

It is worth reminding Bohr's introductory statement to the second volume of the *International Encyclopedia of Unified Science*:

«Notwithstanding the admittedly practical necessity for most scientists to concentrate their efforts in special fields of research, science is, according to its aim of enlarging human understanding, essentially a

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[17] Cf. FAYE, "Niels Bohr and the Vienna Circle", in J. MANNINEN, F. STADLER (eds.), *The Vienna Circle in the Nordic Countries* (Dordrecht: Springer Verlag, 2009), 33-45, p. 33.

unity. Although periods of fruitful exploration of new domains of experience may often naturally be accompanied by a temporary renunciation of the comprehension of our situation, history of science teaches us again and again how the extension of our knowledge may lead to the recognition of relations between formerly unconnected groups of phenomena, *the harmonious synthesis* of which demands a renewed revision of the presuppositions for the unambiguous application of even our most elementary concepts. *This circumstance reminds us not only of the unity of all sciences aiming at a description of the external world but above all, of the inseparability of epistemological and psychological analysis*. It is just in the emphasis on this last point, which recent development in the most different fields of science has brought to the foreground, that the program of the present great undertaking distinguishes itself from that of previous encyclopaedic enterprises, in which stress was essentially laid on the completeness of the account of the actual state of knowledge rather than on the elucidation of scientific methodology.

It is therefore to be hoped that the forthcoming *Encyclopaedia* will have a deep influence on the whole attitude of our generation which, in spite of the ever increasing specialization in science as well as in technology, has a growing feeling of the mutual dependency of all human activities. Above all, it may help us to realize that even in science any arbitrary restriction implies the danger of prejudices and that our only way of avoiding the extremes of materialism and mysticism is the never ending endeavour to balance analysis and synthesis»[18].

In this statement Bohr only apparently seems to outline the methodological manifesto for a neo-positivistic research program. In fact, while he shared with them that science's goal is an essential unity that aims at the description of the external world, he also paid attention to the inseparability of epistemological and psychological analysis. Why did he emphasize this last point? Bohr's stress on the relations between epistemology and psychology is nothing but a differentiation from the previous encyclopaedic enterprise that the logical positivists laid down. Bohr's insistence on the balancing between

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[18] BOHR, "Analysis and Synthesis in Science", *International Encyclopedia of Unified Science*, 1938, 1: 28. Italics are of the present author.

analysis and synthesis means that, for him, the intelligibility of the theory counts as much in science as the data of experience.

The logical positivists or logical empiricists' main purpose was the synthesis of all the elements that contributed to a program of making a philosophy scientific. They stated five basic goals.

The first was the goal of eliminating the claim to legitimacy any form of knowledge that was neither empirical nor mathematical. According to the second goal, only those claims that could be verified on the basis of sensory perception were admissible. Third, they took theoretical physics to be the paragon of empirical knowledge. Fourth, all genuine knowledge should be unified into a single body of knowledge on the basis of a neutral observation language. Fifth, all practical issues (ethics, politics, aesthetics and so on) were to be considered as utilitarian matters to be decided on the basis of Bentham's calculus of utility. The unity of science was taken in two different senses: as a logical unity and as an "encyclopaedic unity". According to the first sense (Carnap), whatever discipline was to be considered derivable from physics. According to the second sense (Neurath), the unity of science was regarded as a commitment to sharing the results of scientific investigation both between scientists in different fields and between scientists generally and the public.

As we have seen, Bohr's concept of unity of knowledge differs from Carnap and Neurath's. In particular, as Faye noted[19], Bohr's concept of unity was not grounded in a reductivist approach. Instead, he had the idea that holistic descriptions of an organism, a mind, or a culture was not reducible to any common physical-chemical description or any other low-level descriptions.

[19] FAYE, "Niels Bohr" (cit. note 17), p. 39.

Bohr met Neurath for the first time in 1934, two years before the Second Unity of Science Congress in Copenhagen, when Jorgen Jørgensen twice that year invited Neurath to Copenhagen. The first time was around April 6, and the second time from October 18-24. During the latter visit Neurath gave six lectures on issues concerning epistemology. In a letter to Rudolph Carnap on November 14, 1934, he tells Carnap that Bohr participated in two of the meetings. He describes the impressions Bohr had made on him as follows:

«Bohr. Idiosyncratic. An intense Man. Came to two lectures and joined the discussion enthusiastically […] Basic line: he does not want to be considered a metaphysician. And he is able to express himself relatively non-metaphysically, when he is careful. Yet obviously there lies a certain tendency in the selection of problems, insofar as the question of life, etc is discussed, as well as in the stress on uncertainty. In addition, his printed remarks are full of crass metaphysics. But he possesses certain basic attitudes which agree with mine, e.g., that in science one cannot clear up everything at once, but that the individual scientific-logical actions have to pay a price, as it were. An idea of compensation, which with him naturally tends to be connected with the uncertainty relation. Obviously tries to come into agreement with us. But since his circle confirms him in his habit to express himself somewhat unclearly, one would have to be able to work on him for a long time, which he would be prepared to do»[20].

According to some notable interpretations (Faye and Folse), it seems reasonable to assume that it was Bohr's talk about the disturbance of the same object, which Neurath

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[20] «[...] Also erst Kopenhagen [...] Bohr. Einzigartig. Intensiver Mensch. Kam zu zwei Vorträgen und diskutierte mit vollem Eifer. Und zwar Rede und Gegenrede. Es interessierte alle sehr -- ausserdem diskutierte man des Nachts wieder. Grundzug: Er möchte nicht als Metaphysiker eingeschätzt werden. Und er kann, wenn er vorsichtig ist, sich relativ metaphysikfrei ausdrücken. Aber offensichtlich liegt in der Auswahl der Probleme, soweit die Frage des Lebens usw. erörtert wird, und die Betonung der Unbestimmtheit eine Tendenz. Überdies sind die gedruckten Ausführungen voll derber Metaphysik. Aber er hat gewisse Grundeinstellungen, die sich mit meinen berühren, z. B. dass man nicht alles gleichzeitig wissenschaftlich aufhellen könne, sondern dass die einzelnen wissenschaftlich-logischen Aktionen sozusagen einen Preis zahlen müssen. Eine 'Kompensations'-Vorstellung, die jetzt natürlich bei ihm tendiert sich mit der Unschärferelation zu verbinden. Offenbar bemüht mit uns in Einklang zu kommen. Aber, da sein Zirkel ihn in seiner etwas unbestimmten Art sich zu äussern bestärkt, müsste man ihn lang bearbeiten können, wozu er sich bereit finden würde." Letter from Neurath to R. Carnap, November 14, 1934, RC-029-10-10, University of Pittsburgh. From FAYE, "Niels Bohr" (cit. note 17) p. 34.

did not like. Such a way of expressing the matter makes it sound as if the object, considered as a thing-in-itself, has some properties which it possesses independently of interaction, but which it is not able to show when it appears as a thing-for-us- or a phenomenon. It is very likely that Bohr and Neurath discussed about these issues, as well as it was just this impression, apparently, which Einstein had received through his conversations with Bohr and by reading his papers, and he therefore thought that Bohr's interpretation attempted to cloak an incomplete description of these things-in-themselves.

In truth, Bohr sustained that phenomena involve things and there is mutuality between observable and observer in the whole process of observation. This is what Einstein probably could not accept. Weizsäcker underlined this point as evidence against the "charge" of positivist addressed to Bohr:

«The fact that classical physics breaks down on the quantum level means that we cannot describe atoms as "little things". This does not seem to be very far from Mach's view that we should not invent "things" behind the phenomena. But Bohr differs from Mach in maintaining that "phenomena" are always "phenomena involving things", because otherwise the phenomena would not admit of the objectification without which there can be no science of them. For Bohr, the true role of things is that they are not "behind" but "in" the phenomena»[21].

Furthermore, Heisenberg recalled Bohr commenting on positivism in 1952 as follows:

«I can readily agree with the positivists about the things they want, but not about the things they reject […] Positivist insistence on conceptual clarity is, of course, something I fully endorse, but their prohibition of any discussion of the wider issues, simply because we lack clear-cut enough concepts in

[21] CARL F. V. WEIZSÄCKER, *The Unity of Nature* (New York: Farrar Strauss Giroux, 1980), p. 185.

this realm, does not seem very useful to me – this same ban would prevent our understanding of quantum theory»[22].

In fact, Bohr justified the complementary application of otherwise contradictory concepts for an exhaustive account of observations at the quantum level.

As John Honner noted[23], it is clear that Bohr cannot be numbered among the phenomenalists. In fact, the theoretical framework of complementarity is as essential for Bohr as whatever experiential content we may order through it. Moreover, complementarity entails at best a very critical phenomenalism, since it insists that one set of experiential data is insufficient for a proper account of experience. Similarly, Bohr's account of symbols and visualizables runs counter to reductionist versions of positivism. According to this view, theoretical terms have no meaning of their own except in so far as they are reduced to observables. While Bohr agrees that descriptive terms only acquire their meaning from macroscopic circumstances, he also allows for the possibility of using such terms as "abstractions" and symbols.

Furthermore, according to the instrumentalist refinement of positivism, scientific theories do not either explain or describe reality, but merely offer a means of predicting results. Bohr's probabilist interpretation of quantum calculations indicates a measure of agreement with the instrumentalist position, but he would not accept the irreconcilable separation of theory and reality which instrumentalist implies.

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[22] HEISENBERG, *Physics and Beyond* (London: Allen and Unwin, 1971), pp. 208 f.

[23] Cf. HONNER, *The Description of Nature* (cit. note 40, ch. 3), p. 173.

Chapter 5

Harald Høffding's Theory of Knowledge

5.1 The Philosophy of Harald Høffding

The early nineties reached the climax of a debate that arose in Denmark at the end of seventies about the historical significance of the cultural relationship between one of the founders of quantum mechanics, Niels Bohr and his mentor and teacher of philosophy, Harald Høffding.

The debate was not only a historiographical dispute about Niels Bohr's history and life, but also a way to show how philosophical influences, and more in general external factors, may shape the scientist's outlook on the world and consequently the approach to his field of studies.

While Jan Faye argued that Bohr was strongly influenced by Høffding in his work on the interpretation of quantum mechanics, David Favrholdt criticized his conclusions. Favrholdt admitted Bohr's cultural debt towards authors such as Poul Martin Møller and Hans Christian Ørsted, but he was not inclined to accept Høffding's influence on Bohr. In more general terms, it is my view that Harald Høffding contributed to the creation of Bohr's *Weltanschauung*, in the light of which his revolutionary scientific research program developed. Moreover, it will be shown that Høffding's philosophy, known as critical monism, is hinged on the concepts of unity and continuity, which also represent the cornerstone of the idealistic interpretation of Niels Bohr's philosophy.

This chapter is an attempt to provide an overview on Harald Høffding's thought and philosophy.

Harald Høffding (1843-1931) enrolled at the University in 1861 and he chose theology for his subject. He attended the lectures of Frederik Christian Sibbern (1785-1872) on logic and psychology and those of Rasmus Nielsen (1809-1883) on a general introduction to philosophy. After taking the examination in philosophy in 1862 Høffding sustained his interest in the subject by attending lectures given by Rasmus Nielsen and Hans Brøchner (1820-1875), who was his third teacher of philosophy and who succeeded Sibbern as professor in 1870.

The philosophy of Sibbern was formed in the main by his attitude toward and criticism of Hegel's philosophy. Sibbern aimed at developing a metaphysics, which was an alternative to that of Hegel both with respect to his philosophy of nature and his psychology. He rejected Hegel's idea that existence is identical with the absolute spirit. In psychology Sibbern's approach was that of seeing the mind as a totality. Classifying mental capacities under the heads of cognition, emotion and will, he believed that mind and body were separate effects of one and the same cause. The tripartition of the mind and the view of mind and body as two aspects of the same substance or process, at that time called the identity hypothesis, were theories Sibbern had passed onto him by his teacher Niels Treschow (1751-1833), who had acquired these ideas from Kant and Spinoza, respectively. Treschow had regarded the will as the most fundamental and original of the three. He had also posited a principle of personality which stated, contrary to the ideas of British empiricists, that the mind consists of a nucleus which figures in the laws of association. Each person has to be regarded as a unity, according to Treschow, a view we encounter in Høffding's thought.

In 1880 Harald Høffding was appointed reader at the University of Copenhagen and three years later he was elected as professor of philosophy. Kierkegaard's thought had a great influence on Harald Høffding in a very critical period of his life, when as a student he underwent a religious crisis. After a hard struggle he agreed with Kierkegaard that the compatibility of science and faith was an illusion as it was not possible to harmonize the demands on the individual made by Christianity with a life moulded by family and state, art and science[1].

Høffding wrote in his *Memoirs*:

«The study of Kierkegaard introduced me to an idea which subsequent philosophical studies led me to amplify and give particular application. This was the notion that the formal feature of the life of the mind is to be found in the unitary and convergent aspects of its synthesizing of experience. And that the measure of an intellectual life resides in the relation between the compass of its content and the dynamism with which this is brought into focus»[2].

By the middle of 1870s Høffding reached a position, which he characterized both as critical positivism and as critical monism. This position supplied him with a philosophy, which could fill in the gaps between science, art and religion. In 1874 he gave a lecture as regards the philosophical problems as he saw them at the time. The lecture is a philosophical manifesto for Høffding's future work. He focused on four issues: 1) the problem of mind (the psychological problem). 2) The problem of knowledge (the logical problem). 3) The problem of existence (the cosmological or metaphysical problem). 4) The problem of evaluation (the ethical-religious problem).

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[1] FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 5.

[2] HØFFDING, *Erindringer* (Mémoires), (Copenhagen: Gyldendalske Boghandel-Nordisk Forlag, 1928), p. 51.

Høffding deemed that these four problems were the same aspect of one, which he identified as the relation between unity and plurality, connection and singularity, or between continuity and discontinuity. This conception derives from Kierkegaard's principle of personality and the tradition of Comte and Spencer. Kierkegaard characterizes the mind as unification of experience and emotion, whilst for Comte and Spencer the nature of mental process is that of aiming at synthesis. Cognition tends to harmonize the maximum number of phenomena with respect to a certain principle or theory. Ethics aims to create a stable personality in harmony with the demands of common good[3].

From 1875 to 1887 Høffding worked on psychology and ethics, from 1887 to 1895 on the history of philosophy, and after 1895 on the philosophy of religion and epistemology. Psychology was the first fundamental problem to which he applied himself from 1875 to 1882, when he published a survey of psychology entitled *Psykologi i Omrids på Grundlag af Erfaringen* (An Outline of Psychology on the Basis of Experience). It was translated in the course of the following years into German, French and English.

It is worth noticing that even though Høffding based his exposition on experience he was very critical of the use of the experimental methods in psychology, holding that the experimental set-up intrudes too much upon the subject of investigation. Moreover, Høffding regarded associationist psychology with scepticism, opposing, for instance, any attempt to reduce associations of similarity to association by contiguity. He anticipated, maintaining this view, Gestalt psychology. In his "law of relation" he stated

[3] FAYE, *Niels Bohr* (cit. note 27, ch. 1), pp. 7-8.

that every element and every state of the mind is determined by the connections into which it enters together with other elements or states of the mind.

In the same year of the *Outlines*' publication he spoke in "*Studentersamfundet*", the newly founded society of students, giving a talk entitled "Om Realisme i Videnskab og Tro" (On Realism in Science and Faith), which was later published in the collection *Mindre Arbejder* (Minor Works), part I. Here Høffding seems to embrace realism, which he defined as the principle of natural causes. Such a realism always deduces certain phenomena of nature from other phenomena of nature, the more complex phenomena from the more simple. Realism in this sense claims "that true knowledge does not consist of accumulated experience but is insight into the interrelationships between experiences". This assumption brings realism closer to idealism, according to Høffding, since one of the essential ideas behind idealism is that we must not accept phenomena as single, isolated facts but that we have to find a bond which brings connectedness and unity.

Høffding's second main work, *Etik. En Fremstilling af de etiske Principper og deres Anvendelse paa de vigtigste Livsforhold* (Ethics. An exposition of the ethical principles and their application to the chief circumstances of life), was published in 1887.

Between the years 1887-1895 Høffding carried on the studies of the history of philosophy. His main work is *Den nyere Filosofis Historie. En fremstilling af Filosofiens Historie fra Renaissancens Slutning til vore Dage* (A history of modern philosophy. A sketch of the history of philosophy from the close of the Renaissance to our own day), which was published in two volumes in 1894-1895.

From 1895 onwards Høffding devoted himself to the philosophy of religion and epistemology. His most important book on religion, *Religionsfilosofi* (The philosophy or religion), was published in 1901.

In 1902 Høffding was elected vice-chancellor for the following year at the University of Copenhagen. In the same year he published his first major work on epistemology, *Filosofiske Problemer* (The problems of philosophy).

Høffding's last major work was *Den menneskelige Tanke, dens Former and dens Opgaver* (Human Thought, Its Forms and Its Tasks), published in 1910.

Høffding attempted to create unity and harmony between opposing views and this position was a reflection of his own personality and character. Moreover, Høffding distinguished himself not much for the originality of his ideas, but rather for the eclecticism of his philosophy and for his personal interests in all kinds of philosophical problems. As a result Høffding contributed to the consolidation of philosophy as a discipline among Danish scholars and scientists.

5.2 The Centrality of Unity and Continuity in Høffding's Theory of Knowledge

According to Høffding's account that arises from *The Problems of Philosophy* of 1902, there is an intimate connection between personal life and scientific inquiry. This connection appears more clearly as we approach the problems of human knowledge, i.e. the problems of philosophy.

The purpose to investigate things is pursuable through "the striving after consistency with one's self under all one's manifold and changing experiences"[4].

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[4] HØFFDING, *The Problems of Philosophy* (cit. note 28, ch. 1), p. 3.

As the theory of knowledge pertains to the validity of our understanding, here arises the problem of the relation between continuity and discontinuity. Høffding noted that what we understand depends not only on the constitution of phenomena, but also on our intellectual organization:

«There is a certain type for all principles and hypotheses, which finally refers back to the innermost nature of consciousness, and here, once more, one comes back to the necessity of unity and continuity»[5].

By unity and continuity Høffding means something like the connectedness of phenomena in forming a unified conception of nature as a whole.

As Kant had shown, all categories can be traced back to the need of unity and continuity. All fundamentals doctrines of natural sciences and logical principles hinge on such a concept.

That demand for unity may have answer in many ways: the only necessary requirement is that the assumptions, which the understanding of the datum calls for, shall be in harmony with the general laws of conscious life.

Professor David Favrholdt pointed out to me that Niels Bohr's conception of nature, i.e. his conception of subatomic phenomena was characterized by discontinuity, because of the quantum leaps, rather then continuity. Therefore, the alleged philosophical similarity between Bohr and Høffding could not hold water.

As I see it, we are facing a double level of analysis: on the one hand, an essential aspect of quantum theory is discontinuity or rupture and it pertains to the very "physical level" of analysis. In fact, quantum postulates express an essential discontinuity, or rather individuality, foreign to the classical theories and symbolized by Planck's quantum of

[5] *Ibidem*, p. 74.

action. On the other hand, Høffding was dealing with the general problem of human knowledge, which depends on our faculty to grasp the interconnections of phenomena in a unified conception of the whole. This is what Høffding meant when he treated the concept of unity and continuity.

Hence, I am convinced that the principle of unity (and continuity), according to the second, gnoseological, level of analysis, is consistent with Bohr's decision to carry on his program in spite of the contradictions with the classical physics in the attempt to reach a unity behind the diversities. At this stage (1912-13), such a concept inspired more Bohr's method of scientific inquiry rather then his conception of subatomic phenomena. A similar interpretation of Bohr's program was proposed some years ago. Ulrich Röseberg noted that if we assume that the physical description of the world of atoms and molecules cannot be based on Newtonian mechanics and Maxwellian electrodynamics, we need a new mechanics and a new electrodynamics. It entails that we will try to find the new theories (quantum physics) by using the well known ones (classical physics) so long as possible. In the meantime we will make only the minimum assumptions, which cannot be formulated in agreement with those theories. According to this interpretation, the philosophy behind Bohr's research program can be characterized in terms of Høffding's as a *philosophy of continuity*[6]. Furthermore, the way in which Bohr used the classical physics and the idea of some correspondence relation with the new ones expresses the concept of continuity.

[6] Cf. RÖSEBERG, "Hidden Historicity" (cit. note 9, ch. 4), p. 331.

Bohr himself in 1929 wrote that the correspondence principle is "our endeavours to utilize all the classical concepts by giving them a suitable quantum-theoretical re-interpretation"[7].

Röseberg stressed the concept of continuity without paying enough attention to the fact that, according to Høffding, continuity is the condition to get the unity of human knowledge.

The forms of perception and the categories of cognition together form the requirement of continuity. As a matter of fact, one of the main themes in Høffding's philosophy was a dualism of continuity and discontinuity, which he claimed underlies every philosophical problem. In his *A History of Modern Philosophy*, when writing about Kant, Høffding gave a characterization of the law of continuity:

«The law of continuity (which includes within both the laws of continuity of space and degree and the law of the causal relations of all phenomena) is valid for all phenomena, because it formulates the general conditions under which we can have real experience (as distinguished from imagination). […] Only as a condition of experience has the law of continuity (including the causal law) validity»[8].

Høffding criticized Kant for making a sharp demarcation between the forms of intuition, the categories of understanding and the ideas of reason, affirming that

«[…] continuity, causality, time and space – as conceived by Kant – possess an ideal perfection to which there is no corresponding experience. Continuity is an idea to which experience only gives us approximations. What Kant calls forms are, as a matter of fact, abstractions and ideals which, in

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[7] BOHR, "Introductory Survey" (cit. note 1, ch. 1), p. 8.

[8] HØFFDING, *Den nyere Filosofis Historie. En fremstilling af Filosofiens Historie fra Renaissancens Slutning til vore Dage* (Copenhagen: Det Nordisk Forlag, 1894-95), translated from the German edition by B. E. Meyer, *A History of Modern Philosophy. A sketch of the history of philosophy from the close of the Renaissance to our own day* (London: MacMillan & Co., 1900), p. 57.

accordance with the nature of our knowledge, we set up and use as measures and rules for our inquiries»[9].

Moreover, Høffding appeared to anticipate Bohr's conception of quantum of action when he gave a definition for the items, or phenomena, which are ordered spontaneously in and for our mind in intuition. Just here one can attain a form of wholeness in intuition. In other words, the phenomena are things or events, physical or mental, which are experienced as immediately given wholes.

Such a whole forms the content of perception in Høffding's view.

Considering the relation between the categories of cognition and such items, Høffding came to regard truth not as a correspondence between certain ideas on the items in the external world and these items themselves.

«Truth cannot be defined as the agreement of our thoughts with reality. We only have knowledge of reality through continual efforts to make the items conform to our forms of thought. Reality, the truth of the items, already consists in practice, for the sound human intellect, in a close connection between as many accurately comprehended items as possible»[10].

He rather figured the truth, i.e. the validity of knowledge, as the greatest possible degree of unity among our representations and the external world in a dynamical and mutually shaping relation.

In his lecture at the Jowett Society in Oxford on November 26, 1904, he expressed his thought on the concept of truth as follows:

«The right to establish something as a principle is founded on the claim that it leads to the recognition of a connection between our observations which would otherwise be obscure and sporadic. The truth of principles, then, does not consist in their conformity to an absolute order of things: - an order of things we

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[9] *Ibidem*, p. 58.

[10] HØFFDING, *Den menneskelige Tanke, dens Former og dens Opgaver* (Human Thought, Its Forms and Its Tasks), (Copenhagen: Gyldendal, 1910), p. 132.

do not know of before findings – with the help of these principles – a connection between our observations. We ourselves produce the truth, when we find the principles, which can connect items to the greatest extent and to highest degree. A critical or dynamical concept of truth is in the making, opposed to the dogmatic concept of truth, which can be designated as static, since it presupposes a given quiescent order of things which is then to be produced in thought. This is nothing new for the philosopher. Critical philosophy had already postulated a dynamical concept of truth, when it pointed out that objective validity consists in the lawful connection of our observations»[11].

By the term "critical philosophy" Høffding refers to Kantian doctrine. Nevertheless, as we have seen in Bohr's conception, Høffding started from a Kantian postulation to arriving at a criterion of reality as the greatest lawful connections between our impressions and our ideas, or as "the constant connection between our observations". Høffding's concept of truth is termed as "dynamical" as it emerges from the activity of the intellect that would never be able to reach a point at which our cognition of the items and the items themselves could be compared. As the same Bohr would later agree because of the element of unity introduced by the quantum of action, foreign to the classical description, whose character of indivisibility is established only insofar as the classical concepts are applied in the description of atomic phenomena. In the lecture given at Harvard University, when he visited William James in 1904, Høffding stated:

«At the present time there is a growing consensus that the significance of scientific and philosophical principles consists in the guidance they give us in our striving towards understanding. Their truth is their validity, and their validity is experienced through their capacity to guide us in our intellectual endeavour. A principle is true if it can be used, that is, if we can work with it to gain knowledge with its aid»[12].

In other words, Høffding regarded the usefulness of scientific principles and theories as

[11] ID., "On Analogy and Its Philosophical Importance", *Mind*, 1905, 14: 199-209, p. 199.

[12] ID., "En filosofisk Bekendelse", in *Mindre Arbejder III* (Copenhagen: Det Nordisk Forlag, 1913), p. 25.

the ultimate criterion of their validity. Their usefulness is revealed by their capacity to bring together as many phenomena as possible in an invariant way. Moreover, he seemed to have found the right balance between the truth-as-coherence and truth-as-usefulness. In fact, Høffding was able to combine the idea of the truth of individual statements with a pragmatic theory of truth. If for the former theory we produce truth when we coherently unite as many as individual statements as possible, at the same time, for the latter we produce truth whether the principles are scientifically fruitful.

One of the main points in common between Høffding, idealism, and Bohr's conception of nature is the so-called criterion of reality. Such a criterion consists in the "lawlike connection" between our observations, which can only be demonstrated if it can be established that there exists a causal connection. The problem emerges as Høffding sometimes refers the concept of "lawful connections" to the nomological laws of a theory, and sometimes to the laws of nature themselves. Nevertheless, Høffding thought that causality is reflected in consciousness as a real unity and there it creates uniformity and continuity between apparently diverse phenomena. The point is not the concept of causality, but rather that Høffding might have conceived a concept whose existence is real and it emerges from the phenomena of nature with the aim to grasp unity among them.

Høffding makes a distinction between the concept of causality and the principle of causality.

The concept of causality is intimately connected with the intrinsic functioning of the mind and it expresses its search for connectedness and unity by relating impressions and ideas. In particular, the concept of causality imparts to the knowing subject a form of thought, which, combined with other concepts dealing with the items existing for consciousness, gives rise to a stable and continuous connection between observations, and this connection constitutes the criterion of reality.

On the other hand, the principle of causality ("that everything has its cause") concerns the universal applicability of the concept of causality and is thus the intellectual expression of the need for continuity. Nevertheless, the principle of causality has not a universal applicability because it is causality as a concept the criterion for what is real.

In the previous chapters I have shown how Bohr's conception of subject/object interaction is consistent with an idealistic and holistic view. As it is evident, we can find such features of holism also in Høffding's philosophy. As a matter of fact, Høffding rejected a representational theory of knowledge, as he claimed that the nature of reality is determined by the entire theoretical description of our immediate experience at any given time. Moreover, since his *The Problems of Philosophy* of 1902, is worth reminding Høffding's stress on an irrational relation that permanently persists between the concepts created by our consciousness and reality itself.

In other words, Høffding was aware of the gap between our experience and the ideal of cognition, which is not able to achieve the universal unity and connectedness of all items. This is to say that there are some individual items, which fail to fall within a causal description of relations between the whole and its parts so that the condition of continuity is no more guaranteed. But Høffding succeeded also in overcoming this difficulty thanks to his conception of nature and knowledge that reveals both epistemic and metaphysical elements of holism.

Professor Faye shed light on an unnoticed dualism as regards the concept of unity in the light of Høffding's holistic account that emerges from his work of 1917, *Totalitet som Kategori*.

Faye argued that Høffding distinguished two different applications of the concept of wholeness.

1. The concept finds a use when the conception of a unity is regarded as the aim of science, in virtue of its search for interconnections between different series of causal connections. After the process of reflection has separated the immediately given, i.e. the items or the phenomena, into their various elements, it synthesizes them once more into new wholes, which until then may have been unapprehended. This happens every time reflection brings about the subsumption of the elements into lawful connections or by articulating universal laws, which may explain singular causal connections. Thus, new items will continually present themselves to thought, creating new task for the cognitive faculty. It is a process which will never come to an end, for what has hitherto been synthesized with new elements. In this context we are using the concept of whole as an idea in the Kantian sense of the world. Høffding holds that this sense is not applicable to what exist in intuition or perception or what can be grasped with any finality in thought. Nevertheless, the concept of totality in this sense is a borderline concept. This may not merely be characterized as a specific category or form of thought but as a feature, which belongs to each of our categories of cognition or forms of thought.
2. The other use of the concept of whole applies to what exists in intuition as immediately given items. In that case it is not the work of reflection, which produces the idea of wholeness, but certain items themselves which invariably appear to us as immediately given wholes prior to any work of reflection.

> Høffding believes that we do find reference to such items in the field of psychology, biology and sociology[13].

Returning to Bohr, I would like to call attention to the double meaning that he himself ascribed to the concept of unity. In particular, on the one hand is worth reminding Bohr's belief in the unity and harmony of nature as a condition for objective knowledge: it could be compared to Høffding's concept of causality, which was regarded as a unity among apparently diverse empirical items. This pertains also to the aim of science as explained above at point 1, and it exerts a heuristic value.

On the other hand, as we have already noticed, like Høffding, Bohr spoke of unity as wholeness or atomicity of an objectively given item, i.e. the quantum of action, perceived by an observing subject. Such a meaning is strictly related to Bohr's conception of subatomic phenomena as it has been already shown on chapter 3.

[13] FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 91.

Chapter 6

Niels Bohr's Cultural Milieu

6.1 Niels Bohr's Family and the Years of His Youth

Niels Bohr descends from a family of a high intellectual standing. He was also a fourth generation university man. Peter Georg Bohr (1776-1847), Niels Bohr's great-grand-father, was first a teacher at the Grammar School in Elsinore and later at the Grammar School in Nakskov. In 1818 he was appointed head-master of the Grammar School in Rønne. He also became *candidatus theologiae* and wrote some essays on history, a Latin textbook and some occasional poems. His son, Henrik Georg Christian Bohr (1813-1880), became *candidatus theologiae* in 1837 and in 1844 headmaster of the von Westen Institute in Copenhagen, a private Grammar School. After his retirement in 1844, he was appointed professor in Copenhagen[1]. Furthermore, Niels Bohr's father, Christian, was professor of physiology at the University of Copenhagen. Christian distinguished himself for his scientific achievements and was one of the pillars of intellectual and cultural life in Copenhagen at the turn of twentieth century. Moreover, he founded the Akademisk Boldklub, the university football club, and thus contributed to spread football as the national sport of Denmark. In 1880 he obtained the doctoral degree and after some years as an assistant professor he became professor of physiology in 1890.

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[1] Cf. FAVRHOLDT, *Niels Bohr* (cit. note 21, ch. 1), p. 8.

In politics he was a liberal in his view: in 1884's parliamentary elections he supported a liberal candidate instead of the conservative, his maternal uncle, Christian Rimestad. He also strongly advocated equality for women. Christian Bohr married Ellen Adler (1860-1930) in 1881.

Ellen Adler was the daughter of D. B. Adler, a banker and a financier who had founded the Commercial Bank of Copenhagen and been an initiator of the Jutland Provincial Credit Association. He was interested in politics and been elected to parliament as a member of the National Liberal Party[2]. Christian and Ellen lived at first in the Adler town house, 14 Ved Stranden, next to Christianborg Palace, the seat of the Danish Government. Here in the heart of Copenhagen the Bohrs' first child, a daughter Jenny was born, and on October 7, 1885, their first son Niels Henrik David, and two years later their second son Harald. At the side of Christian Bohr Ellen became the centre of the constantly growing circle of intellectuals who were guests in their lovely home in the old surgical academy in Bredgade, where the Bohrs moved in and took up the residence when Christian became professor of physiology, as did all Danish professors in order to stay in a house adjacent to their own laboratory and classrooms[3]. At the time of Ellen's death, Niels Bohr's close friend from his childhood, Ole Chievitz, later professor of surgery at the University of Copenhagen, described her as follows:

«And Ellen Bohr's lovable personality cast its warm glow over everything, for this was the essence of her nature. It was so great that I can imagine that people who met her for the first time through it must be an

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[2] Cf. ROZENTAL, "Childhood and Youth", in *Niels Bohr, His Life* (cit. note 2, ch. 4), pp. 12-13.

[3] Cf. RUTH MOORE, *Niels Bohr: The Man, His Science, and the World They Changed* (New York: Alfred A. Knopf, 1966), p.8.

affectation; but one did not have to be with her many times to discover that like everything else about Ellen Bohr it was genuine, honest and strong. It was an unselfishness second to none […]»[4].

At the age of seven, the age at which all Danish children entered school, Niels went to the Gammelholm School. At the time, school was disciplined and formal, as Niels Bohr's class was still subjected to the old "Education Act" that was replaced only in 1903. Notwithstanding the educational severity, school presented no problem for the young Niels, who could also benefit from his father's teaching and education. It is beyond doubt that Niels and Harald Bohr grew up in a home that was distinguished in outlook, humanity and of course in intellectual qualities. Christian Bohr took care of his children's education and wanted to make certain that they appreciated the beauties of nature. He recalled an episode in one of their walks they were used to make. He called Harald and Niels' attention to a tree-see how beautifully the trunk divides into branches and the branches into twigs and how the leaves bud at the end of the twigs. Niels listed intently and answered: "Yes, but if it weren't like that it wouldn't be a tree"[5].

Harald Bohr was generally regarded as the brighter of the two boys, but Niels was "the special one of the family", as Christian expressed it, and from his earliest youth Harald agreed with this.

Harald became an almost internationally famous among mathematician as Niels was among physicists. However, through the reminiscences from Niels' childhood emerges like a leitmotiv the inseparability between the two brothers.

Returning to Professor Christian Bohr, he was also a disciple of Goethe and could recite whole sections of Faust from memory. Niels, as he walked at his father's side or sat at

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[4] ROZENTAL, "Childhood and Youth" (cit. note 2), p. 11.

[5] MOORE, *Niels Bohr* (cit. note 3), p. 9.

his feet on long winter evenings, learned Goethe almost by absorption. To only a slightly lesser degree, Professor Bohr admired Shakespeare and Dickens and frequently gathered the children around him to hear these authors[6].

The children also had the opportunity of an early exposure to the best scientific and philosophical thought of Denmark. As a member of the Royal Danish Academy of Sciences and Letters, Professor Bohr, together with the professors Harald Høffding and Christian Christiansen, later Niels' teacher in physics at the University, formed the habit of stopping at a café to continue the discussions of the meeting or to discuss their own deep interest in the relation to science to life. As the first meetings take place in cafes, the three professors soon decided to shift to their homes for dinner every other Friday night. It was when the famous philologist Vilhelm Thomsen joined the trio. Harald Høffding mentioned these meetings in his Memoirs:

«Now at some point in the period I have been talking about [around 1893] regular gatherings began to take place which I have taken and still take great pleasure in. It all started through my habit of joining the physiologist Christian Bohr after the meetings in the Royal Academy, often continuing my conversation with him in a café where we had supper. I had already been in touch with Bohr earlier, who at that time had learned that I was working on a treatise on psychology in which I intended to incorporate as many physiological views and results as possible. He offered his assistance by reading the relevant part. These evening visits which were thus initiated were followed by many more. In his and his wife's beautiful home in the stately residence of the professor of physiology I have spent many interesting and enjoyable evenings. Being a follower of the Leipzig scientist Ludwig, Bohr belonged as a physiologist to the movement which strongly insists on physical-chemical methods in physiology. […] Before long the physicist Christiansen was also one of the party at our café life, and so it was that we in turn went back to

[6] *Ibidem.*

the home of one of us those Friday evenings when meetings in the Academy were held. Furthermore we gained a fourth member in person of the famous linguist Vilhelm Thomsen»[7].

The gatherings began to take place around 1893[8], when Niels Bohr was eight years old. In his commemorative speech on Høffding, in 1931, Bohr junior mentioned those evening's meetings at Bohrs' home, when he and his brother Harald were given the opportunity to participate in as listeners:

«My first recollections of Høffding stem from some evening gatherings – described by himself in his Memoirs – when a small circle of scientists about a generation ago met regularly in each other's homes to discuss all kinds of questions which had caught their interest. The other members of this circle were close friends of Høffding's from their student years together, Christian Christiansen and Vilhelm Thomsen, plus my father who was a good deal younger, but whose friendship for Høffding over the years increased in intimacy. From the time that we were old enough to profit from listening to conversations and until these gatherings in our home came to an end upon the early death of my father, we children were allowed to be present when the meetings were held at our house, and they left us with some of our earliest and deepest impressions. During the discussions, which were often very lively, Christiansen especially would tease Høffding in his typically good-natured way about philosophy's general aloofness from the world; but like everyone else present he was well aware of exactly to what extent Høffding's ability to understand and the impetus he had towards forming a general synthesis of differing points of view was, so to put it, the nourishing soil from which the ideas of the others sprouted, though marked by their diverse academic backgrounds and views of life»[9].

It seems that Christian Bohr continued to take part in these meetings until he died, in 1911. From the last quotation we also get the impression that Høffding occupied a leading role in directing the discussions.

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[7] HØFFDING, *Erindringer* (cit. note 2, ch. 5), pp. 171 f.

[8] *Ibidem*.

[9] BOHR, "Mindeord over Harald Høffding" (Harald Høffding in memoriam), *Oversigt over Det Kgl. Danske Videnskabernes Selskabs Forhandlinger 1931-1932*, p. 131.

A second allusion to these meetings is to be found in Niels Bohr's paper, "Physical Science and the Problem of Life", written in 1957. In this paper he dealt with the complementarity of mechanical and teleological considerations in biology.

«I have quoted these remarks which express the attitude in the circle in which I grew up and to whose discussions I listed in my youth, because they offer a suitable starting point for the investigation of the place of living organisms in the description of nature»[10].

The quote Bohr refers to in the passage above was of his father's paper, published in the *Festschrift* of the University of Copenhagen. According to the previous quotation, the group used to debate mechanism and vitalism in biology. It is also worth remarking that Høffding believed that biological organisms possess individuality or a wholeness, which could not be explained by reducing this totality to the sum of all its elements.

Høffding considered biology to be another field of science where the object is experienced as a whole, as can be deduced from his essay published in 1925, "The Theory of Knowledge and Apprehension of Life" concerning the problem of description in biology:

«From the aspect of epistemology, in my opinion, the matter is such that no limit can be set once and for all to the use of physical and chemical points of view in the organic field, but then such an application cannot in fact be carried out. The problem of the relation of an organic whole to its conditions, to the individual processes, on whose collaboration the life of the whole exists, can neither be solved by proclaiming mechanism as the solution to all riddles, nor by letting the whole "itself" intervene as a deus ex machina»[11].

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[10] ID., "Physical Sciences and the Problem of Life", in *Atomic Physics* (cit. note 43, ch. 1), 94-101, p. 96. Reprinted in *The Philosophical Writings of Niels Bohr*, 2 (cit. note 46, ch. 3), 94-101.

[11] HØFFDING, "Erkendelsesteori og Livsopfattelse" (The Theory of Knowledge and Apprehension of Life), *Det Kgl. Danske Vid. Selsk. Filosofiske Meddelelser II*, 1925, 1, p. 3. Cf. FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 103.

Even though from the historical sources it is difficult to say exactly who influenced who, a conceptual symmetry is perceivable between Bohr senior and Høffding respective positions on biology.

6.2 The Biological Conceptions

Christian Bohr's untimely death at the age of 56, in 1911, was a great loss for the Danish intellectual community. Høffding gave a commemorative speech, which was published in the journal *Tilskueren* (The Spectator):

«This man was carried off while at the height of his powers. Shattering and overwhelming was the news of his death. It was a happy death, indeed; he had just returned from work and went to the bed which was within a few hours to become his deathbed. To fall asleep in this way after a day's work is what many people could wish for. But in this case the words of the poet hold no value, "He fell asleep like the sun sets in fall". For it was not fall, not autumn, for him yet. He was not yet one of the veterans. He was as yet in the summer of life, still displaying in the searching ingenuity of his mind and the energy present in all his labour that he was first in rank among Danish scientists and with many years of fruitful work in prospect. If it has fallen to my lot to speak here, it is not because I was a fellow-worker in his field. But I have followed the course of his work over a number of years with profit and admiration. He offered me a helping hand when I needed assistance for my own work from a specialist in his field, and it was this that first brought us closer together. Later I had the opportunity to follow his work over a number of years in the Royal Academy of Sciences and Letters. Whenever he began to speak, the evening became a festive one, all absorbed by the account of his new discoveries, the difficulties which he had overcome and the new problems which had emerged for him. For many years his research traced a singled fixed line. In one of the most central areas of research, that organic life, it was his task to investigate the boundary between life and the forces of inorganic nature – to discover whether the boundary was unmovable, and if so, where it was located. And for such an investigation the physiology of respiration was especially fitted. Bohr stands out as an independent figure in the era of the history of physiology of respiration which was founded by Johannes Müller and his disciplines in Germany, and by Claude Bernard in France, and whose program was to trace the general effects of natural forces as far as possible in the process of organic life. Bohr brought to light a provisional limitation of their action by showing that respiration was not entirely determined by the influence of external conditions. The enigma turned out to be located further back than the conflicting parties believed. There was a time when Bohr complained of his position being misunderstood, it being thought that he wanted to regress to so-called vitalism. But his opinion was merely that a new chain of investigation had to be established, especially on the influence of the nerve system on the processes in the lung cells. His position here shows something of his character as a scientist, and in his last dissertation, published in the Festschrift of the University, he presents a clear exposition of his view. The premium he set on thought shows itself in his critical grasp of a range of problems, paying attention to the vast horizons which in the wake of every new chain of investigations are revealed to the true scientist – but at the same time with the firm conviction that only one road is given along which progress in the new areas can be made: unrelenting work, faithful to the spirit of stringent science. His vocation could have as a motto Lotze's dictum, "Only exhilarated by the great, but faithful to the small". He was for us all an instructive example, not only in virtue of the results his research brought forth, but more especially through his stature as a scientist.

He has done honour to his country. May our flag not be flown victoriously in the strife of the world powers, but may it be proudly flown above works of the mind, above achievements of thought, and may it be lowered at his bier.

I have been requested by the president of the Royal Academy and the vice-chancellor of the University to express our deceased colleague for everything he has meant to the scientific community at home and to the training of scientists. He was a colleague whose words had weight. His acute intelligence penetrated many obscure connections and his unfailing common sense was often salutary. The status of science at home was close to his heart. He had a sharp eye for the dangers which might menace within the limited horizons of a small country and for the rise of popularization which often lets undigested knowledge come to the fore as allegedly true knowledge. In his own untiring work he set a living example of combat against such dangers.

For those who were not colleagues but had the pleasure to be his intimates, his personality in private life displayed the same qualities as those which characterized him as a scientist. His free and vigorous mind often grappled with paradoxes, and he displayed his wit; facing his criticism, friends often could find themselves in a purgatory, but behind his criticism lay a respect for serious work and great fidelity in friendship. He was not of their number who allow themselves to be overwhelmed by feelings, or who offer or expect declarations of friendship, but such reticence gave force to the effect of a warm glance and his firm handshake. Neither was he of their number who make a habit of speaking about their philosophy of life and destiny. Deep within him lay a view which could be said to bear a resemblance to Goethe's lofty view of life, at once both naturalistic and idealistic, in which all that was petty and bitter faded into far horizons. There were few who knew their Goethe as he did. The lines of Mephestole were perhaps those he most often quoted; behind them lay an aspiration like that Faust, only with the difference that he did not, unlike Faust, relinquish the work with flasks, levers and screws (Helbeln und Schrauben); the path to truth for him was not to be found outside the scientific laboratory.

Now we shall no longer see his bright eyes and fine, intellectual forehead which distinguished his sturdy figure; or listen to his instructive expositions or to his wit and sharp criticism.

His loss is a great one for science at home and abroad and for the circle of friends and colleagues. But we to whom he meant so much, however, turn now to the one and to those for whom he meant most. His personality pervaded his beautiful home through his life and his interests, and those to whom he opened it will understand how great the loss there must be felt. There he found love and understanding. In this as in other things he was a happy man. Not least in the pleasure he had of seeing young, capable persons develop in just those areas of scientific work in which he was to have a happy working evening of life together with his ambitious sons and with the comfort of all the love which surrounded him at home. But it was not be so.

But from the thought of the suddenness of the loss and of all which the loss implies we return again to the memory of his personality and his stature as a scientist.

Honoured be the memory of Christian Bohr»[12].

Christian's Bohr paper, which Høffding is speaking of is the very same essay that Niels Bohr cited in 1957. It was Bohr senior's aim, as a follower of Johannes Müller and

[12] ID., "Mindetale over Christian Bohr" (Commemorative speech on Christian Bohr), *Tilskueren*, 1911, 209-212, p. 211. From FAYE, *Niels Bohr* (cit. note 27, ch. 1), pp. 14-15.

Claude Bernard, to use mechanical ideas as far as possible in the description of living organisms.

Christian Bohr claimed, as did Høffding also, that the nature of living organisms could not be explained only in terms of a mechanical description. Niels considered this period as essential for his education as the frequent references to biology in his later philosophical writings clearly show.

The discussions which Niels and his brother Harald were allowed to listen to were the occasion in which both they absorbed an interest in the debate over mechanistic versus vitalistic descriptions[13]. As we have seen, Bohr recalled these discussions in the paper "Physical Science and the Problem of Life" from 1957, after quoting a passage from the introduction to his father's paper "On the Pathological Lung Expansion", which appeared in the anniversary publication [*Festschrift*] of the Copenhagen University in 1910.

Here Christian Bohr[14] sustains the heuristic value of teleological considerations in addition to purely mechanical considerations in biology.

«[…] The a priori assumption of the purposiveness of the organic process is, however, in itself quite natural as a heuristic principle and can, due to the extreme complication and difficult comprehension of the conditions in the organism, prove not only useful, but even indispensable for the formulation of the special problem for the investigation and the search of ways for its solution. But one thing is what may be conveniently used by the preliminary investigation, another what justifiably can be considered an actually

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[13] As it is known, Aristotle started to discuss the problems related to the characteristics and the explanation of life. The development of mechanics in the 17th century reduced these problems to the question of whether or not life could be explained in terms of mechanics. The term "mechanicism" or "mechanism" meant that living organisms were nothing but complicated mechanical machines. Mechanism of today, in the light of the development of physics and chemistry, is the view that life can be explained exhaustively in terms of these two disciplines.

[14] Christian Bohr had been taught Kant's view on teleology and causality at the "Filosofikum". His teacher, the professor of philosophy, Rasmus Nielsen, was at the time very interested in precisely these ideas.

achieved result. As regards the problem of purposiveness of a given function for the maintenance of the whole organism, such a result can, as stressed above, be secured only by a demonstration in detail of the ways in which the purpose is reached»[15].

Nevertheless, Christian Bohr's position on the uniqueness of life science and its irreducibility to physical and chemical sciences also found empirical evidence in his physiological research. Bohr senior made the firm claim that lung tissue plays an active part in the gas exchange in the lungs, and that this exchange is not a simple diffusion process. On the basis of experimental work, Christian Bohr concluded, in a detailed review article published in 1909 that "a specific cell activity in the gas exchange in the lungs has been established beyond doubt". He interpreted this finding to mean that the behaviour of lung tissue was regulated according to the needs of the organism as a whole. Lung tissue's behaviour could therefore only be understood in relation to the function to keep the organism alive.

Niels Bohr regarded his father's position and the general attitude of the circle in which, as Bohr himself admitted in the 1957's lecture, grew up as a starting point for the investigation of the place of living organisms in the description of nature. Bohr's initial standpoint on the controversy over mechanism versus vitalism was finally upheld by the modern development of atomic physics, which revealed the limitation in principle of the mechanical conception of nature.

As Harald Høffding was the philosophical prime mover within the circle, it is worth noticing the conceptual parallelism between the way he had analysed the problems of description in biology and Bohr's later formulation of complementarity with respect to biology. Høffding believed that life is a feature, which characterizes organisms as

[15] BOHR, "Physical Science" (cit. note 10), p. 96.

indivisible whole. The relation between the whole and its parts cannot be reduced to a merely sum or aggregation of the elements of the whole.

In the above quoted article, “The Theory of Knowledge and Apprehension of Life”, Høffding made clear that the problem of the relation of the organic whole to its conditions cannot be solved by proclaiming mechanism as the solution to all riddles as far as the analysis of wholeness is irreducible to an analysis of the causal interrelations among the components of the system. For this reason there exists an antinomy between the immediate understanding of the organism in experience as a whole and the organism regarded as description of the causal relations among its parts. Consequently, the understanding of biological organisms can be achieved on premises other than psychochemical ones.

In 1932 Bohr gave for the first time an account of the application of complementarity to biology in an address called “Light and Life” at the International Congress of Light Therapy in Copenhagen. Here Bohr regarded the revision of the foundations of mechanics not only as essential for the full appreciation of the situation in atomic theory, but also as a background for the discussion of the problems of life in their relation to physics.

«In no way does this mean that in atomic phenomena we meet with features which show a closer resemblance to the properties of living organisms than do ordinary physical effects. At first sight, the essentially statistical character of atomic mechanics might even seem to conflict with the marvellously refined organization of living beings. We must keep in mind, however, that just this complementary mode of description leaves room for regularities in atomic processes foreign to mechanics but as essential for our account of the behaviour of living organisms as for the explanation of the specific properties of inorganic matter»[16].

As it is evident, Bohr's interest in biology originated from his youth, but where did the idea of application of complementarity to biology derive from? Was it part of Høffding's cultural legacy or did Bohr develop it autonomously?

It is beyond doubt that much of what Bohr said about biology sounds like an echo of Høffding:

«[…] if we were able to push the analysis of the mechanism of living organisms as far as that of atomic phenomena, we should not expect to find any features foreign to matter […] however […] the conditions holding for biological and physical researches are not directly comparable, since the necessity of keeping the object of investigation alive imposes a restriction on the former which finds no counterpart in the latter […] On this view, the very existence of life must in biology be considered as an elementary fact, just as in atomic physics the existence of the quantum of action has to be taken as a basic fact that cannot be derived from ordinary mechanical physics»[17].

As a matter of fact, Bohr too regarded the existence of life as an elementary fact as well as the quantum of action with respect to physics. Considered the existence of such irrational element, from the point of view of classical mechanics, would not be possible to reduce biology to the physical or chemical explanation.

«Indeed, the essential characteristics of living beings must be sought in a peculiar organization in which features that may be analysed by usual mechanics are interwoven with typically atomistic features to an extent unparallel in inanimate matter»[18].

[16] ID., "Light and Life", in *Atomic Physics* (cit. note 43, ch. 1), 3-12, p. 7. Reprinted in *The Philosophical Writings*, 2 (cit. note 46, ch. 3), 3-12.

[17] *Ibidem*, p. 9.

[18] *Ibid.*, p. 8.

A non-causal feature of wholeness thus characterizes both living organisms and the quantum of action. Certainly, here, “atomistic feature” does not refer to certain properties of atomic systems but to traits of individuality or wholeness, which are also seen in the quantum of action.

As a consequence, mechanistic considerations cannot replace teleological ones, as the vital functions are un-analysable in terms of physical and chemical descriptions. Bohr added: “The concept of purpose, which is foreign to mechanical analysis, finds certain fields of application in biology”[19]. Moreover, the experience of living organisms as wholes demands the use of finalistic concepts in the description of their behaviour. Since the presuppositions underlying teleological and mechanistic descriptions are mutually exclusive, the two types of description cannot be simultaneously sustained and thus do not come into conflict with each other. In this sense Bohr held that the finalistic mode of description is complementary to the physical mode of description, in virtue of the fact that the observational conditions required for each, taken individually, are mutually incompatible. Or, as one might also put it, since the property of life applies to the object taken as a whole whereas a mechanistic description applies merely to the object considered as a consisting of the sum of its parts, mechanistic and teleological descriptions are mutually exclusive and yet may be deployed complementarily.

According to Bohr, biological organisms display such holistic characteristics, e.g., self-preservation and reproduction, that they seem inaccessible to an unambiguous causal mode of description, even though a far-reaching understanding of the chemical and physical aspects of many typical biological reactions has been achieved by now.

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[19] *Ibid.*, p. 10.

«In fact, we are led to conceive the proper biological regularities as representing laws of nature complementary to those appropriate to the account of the properties of inanimate bodies, in analogy with the complementary relationship between the stability properties of the atoms themselves and such behaviour of their constituent particles as allows of a description in terms of space-time coordination»[20].

As Christian Bohr and Harald Høffding had remarked yet, so Niels too sustained a viewpoint lying equally distant from vitalism and mechanism. In “Light and Life” Bohr deemed untenable the understanding of the essential aspects of life in purely physical terms. As well as he held that it was hard to disambiguate the vitalistic view, which presupposed the existence of a vital force unknown to physics.

Conversely, no result of biological investigation can be unambiguously described otherwise than in terms of physics and chemistry. Therefore Bohr’s idea to apply the complementary view to the long-lasting controversy between mechanism and vitalism derived from an analogy he drew between the concept of life as an elementary fact, in biology, and the existence of quantum of action as a basic fact that cannot be derived from ordinary mechanics, in physics. In “Light and Life” as well as in “Biology and Atomic Physics” Bohr used the concept of complementarity in the description of living organisms in a way similar to Høffding’s.

The corresponding viewpoint between Bohr and Høffding is confirmed by the fact that Høffding sustained that no limit can be set to the use of physical and chemical points of view in the organic field. Therefore, mechanism cannot be regarded as the solution to all riddles. The concept of holism cannot be used to give scientific explanation of whatever holds the individual parts together, because “that would be to explain idem per idem”. However, because of the unity and coordination of life, it is necessary to take account of

[20] ID., “Biology and Atomic Physics” (cit. note 47, ch. 3), p. 21.

the viewpoint of holism for an understanding of the relation between the individual processes and the life of the organism as such. Also in Høffding's view life is the factor that makes biology unidentifiable on the basis of chemical and physical concepts. Høffding expressed his view as follows:

«The more rational and formal is our knowledge, the more it can advance through definitions and deductions so that all transactions are made with intuitable necessity. Empirical or actual knowledge, on the other hand, must often halt at facts that could indeed be described and analyzed but not defined and derived from other facts. On such fact is life, and thus biology belongs under, and will probably always belong under empirical knowledge»[21].

As it is evident, the holistic view is regarded as a regulative principle. Hence Høffding wrote:

«The viewpoint of the whole is thus not in absolute contradiction to the mechanical conception. They stand in relation to each other as synthesis and analysis. And the one cannot exclude the other […]»[22].

Høffding reputed as necessary to adopt holistic or teleological considerations in addition to mechanical ones for living organisms.

Furthermore, the concept of life as an elementary fact gives rise to another consideration: the necessity to maintain life in the object under observation implies a significant limitation to our possibilities for investigating these functions. There are two main reasons which could explain why. First of all, every experimental arrangement, utilized for studying the behaviour of the atoms constituting an organism, precludes the possibility of keeping the organism alive. Following Joseph Needham, we may call this

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[21] HØFFDING, "Om Vitalisme" (On Vitalism), in *Mindre Arbejder I* (Copenhagen: Det Nordisk Forlag, 1905), pp. 48-49.

[22] ID., "Totalitet som Kategori", *Det kgl. Danske Vid. Selsk Skrifter*, 1917, Række 6, II, pp. 26-27. From FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 103.

argument the "Thanatological Principle" (from the Greek word for death: Thanatos)[23]. In his later writings Bohr was explicit in holding that the impossibility of keeping a certain organism alive in the course of a process of investigation is rather a matter of empirical fact. But in his 1932 and 1937 articles Bohr stressed the argument that the observational conditions necessary for a purely mechanistic definition of the vital functions are incompatible with those necessary for the manifestation of life[24].

Second, the incessant exchange of matter inside the living beings does not make possible to regard them as well-defined systems of material particles like those considered in atomic physics.

David Favrholdt called it the "Metabolism Argument"[25]. From this point of view, it is meaningless to speak about which atoms form part of the living organism and which do not. Considering an organism as a whole, we deal with observational situations, which do not allow a sharp demarcation between the organism and its surroundings. On the contrary, if we wish to investigate the organism in physico-chemical terms, we need to isolate it from its environment. In the course of time Bohr had realized that both the Metabolism Argument and the Thanatological Principle must be revised even though he did not give them up completely[26].

Henry Folse too has pointed out that in his earliest essays on biology, Bohr disregarded the fact that in quantum physics the impossibility of separating the states of two interacting systems is a physical consequence of the functions of a certain organism in

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23 JOSEPH NEEDHAM, *Order and Life* (Cambridge, MA: MIT Press, 1936).

24 FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 162.

25 FAVRHOLDT (ed.), *Niels Bohr Collected Works* [NBCW], Vol. 10: Complementarity beyond Physics, 1928-1962 (Amsterdam, NL: Elsevier, 1999), p. 11.

26 BOHR, "Light and Life Revisited", in *Essays 1958-1962* (cit. note 24, ch. 1), 23-29. Reprinted in *The Philosophical Writings of Niels Bohr*, 3 (cit. note 24, ch. 1), 23-29.

physical terms and keeping that organism alive as an experimental problem which might be overcome by refined techniques.

Consequently, Folse argues, Bohr did not recognize until much later that the recourse to complementary description is made when we are allowed to describe biological phenomena on two distinct levels[27]. If this claim is true it looks very much as if in the thirties Bohr's conception was characterized by the same ambivalence we observed in Høffding, in the beginning, as to the impossibility of an exhaustive physical description of living things. Folse is correct insofar that in his early essays Bohr did not directly endorse what he explicitly stated in his 1960 article, that

> «The basis for the complementary mode of description in biology is not connected with the problems of controlling the interaction between object and the measurement toll, already taken into account in chemical kinetics, but with practically inexhaustible complexity of the organism»[28].

As is known from the quantum physics, observing and observed systems form an interacting whole with respect to which we can only draw an arbitrary division between subject and object. It is this fact which, for Bohr, requires the complementaristic account in quantum physics.

One therefore naturally gets the impression that Bohr saw a similitude between physics and biology in the sense that he believed there was a similar argument in favour of complementary descriptions in biology[29]. There is not such argument in Bohr's conception on biology.

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[27] Cf. FOLSE, *The Philosophy of Niels Bohr* (cit. note 56, ch. 3), pp. 185 ff.

[28] BOHR, "The Connection between the Sciences" (cit. note 11, ch. 4), p. 21.

[29] Cf. FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 162.

It is the concept of life as an irreducible and unitary element what allows Bohr to draw a parallelism between quantum physics and biology.

Chapter 7

The *Ekliptica*'s Years: Between Friendship and Cultural Dedication

7.1 The "Ekliptica Circle" Origin

Niels Bohr was enrolled at the University of Copenhagen in 1903, when he began studying physics under Professor Christian Christiansen. In 1909 he received his master's degree in physics and two years later he completed his Ph.D. Then he spent a year in England doing a post-doctoral research in Cambridge and Manchester.

At the time, all students had to attend a year-long propaedeutic course in philosophy, called in Danish *Filosofikum*, that was usually taken during the students first year at the University. Bohr attended the mandatory course in 1903 with Professor Høffding.

Since 1905 Bohr began to meet regularly a group of students that was used to discuss on various subjects. In the Niels Bohr Archive, among Bohr's Scientific Manuscripts[1], there is a sheet of paper on which somebody has drawn a hepta-pointed star, in each point of which a name and an address have been written. Five of the names belong to members of Ekliptica. The group was called Ekliptica because the initial number of members was twelve: apart from the Bohr's brothers there were Peter Skov (jurist, later

[1] NIELS BOHR ARCHIVE, *Bohr Scientific Manuscripts*, 5, 5.

Danish Ambassador in Moscow, Ankara, Warsaw and Prague), Edgar Rubin (the psychologist), Poul Nørlund (the historian), Niels Erik Nørlund (the mathematician), Vilhelm Slomann (the art historian), Kaj Henriksen (the entomologist), Einar Cohn (later permanent under-secretary), Lis Jacobsen (the etymologist), Viggo Brøndal (the philologist), Astrid Lund, who later became Lunding by marriage[2]. The two names, which were not included among the previous Ekliptica members, are those of Gudmund Hatt (the ethno-geographer), and Elias Lunding (the agronomist). Bohr's address mentioned in the paper is "Gersonsvej 55", Hellerup, in the suburbs of Copenhagen, where he lived until 1921, when he moved to the Niels Bohr Institute in "Blegdamsvej 17". So the gatherings, which the sheet of paper refers to, took place around 1916-17.

Except for the period September 1911- July 1912 and 1914-16, when Bohr was in England, he continued to participate in the meetings at least until 1917.

Our first knowledge about the group derives from Vilhelm Slomann, who wrote about the club in a newspaper article in 1955, thanks to which we know the exact starting date of the club meetings:

«From 1905 and on, Edgar Rubin, later a Professor of Psychology, for a number of winters gathered twelve students of the same age for meetings of the type students usually hold – consisting of a lecture, a cup of tea at a restaurant or a boarding-house and a considerable consumption of tobacco. The natural and social sciences, geography and the traditional humanistic subjects – philosophy, literature, linguistics, archaeology and history – were represented here, and the various interests of the twelve students were manifested in the pompous name of the club: Ekliptica. Rubin was permanently the vigilant and natural centre in this circle, whose members in due course became professors or in other ways had the opportunity to carry out scientific work. Niels and Harald were frequently present and listened attentively to the various lectures and discussions. When the discussions were dragging to their close, one of them

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[2] ROZENTAL (ed.), "Childhood and Youth" (cit. note 2, ch. 6), pp. 22-23.

would say a few generous words about the lecture and continue in a low voice at a furious pace and with vigorous intensity, but would often be interrupted by his brother. Their way of thinking seemed to be coordinated; one would improve on the other's or his own expressions, or in a heated, yet at the same time good-humoured manner defend his choice of words. Ideas changed their colour and became polished; it was not a question of defending preconceived opinions, but of something new coming into being. This way of thinking *à deux* was so deeply ingrained in the brothers that nobody else could join in. The chairman used to put his pencil down quietly and let them carry on; but when everybody moved in closer to them, he might say ineffectively, "Louder, Niels"»[3].

Among historians the idea is quite widespread that the circle has been of great importance for the formation of Niels Bohr's method of work. Slomann described a picture that calls to one's mind the way in which Bohr developed his thoughts. Bohr needed to create a feeling with the person he talked to, while Harald, his brother, preferred to work alone. For Bohr it was necessary to feel on common ground with those who became his most intimate collaborators in solving scientific and social questions, as it happened with Hendrik Anthony Kramers and Léon Rosenfeld.

Bohr could not work if he did not find in his closest environment the most complete harmony and understanding and he began to experience it since the period of Ekliptica meetings.

To go beyond the considerations on the methodicalness, i.e. the quality of appreciating Bohr's method and system of work, Slomann also tell us that all natural and human sciences were represented in the circle, and Edgar Rubin was its prime mover. But there is no mention about how did they know each other. Peter Skov gave another report on Ekliptica that may shed some light on this issue:

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[3] FAVRHOLDT, *Niels Bohr* (cit. note 21, ch. 1), p.19.

«After leaving school (1901) I began the study of law, partly in order to please my father, partly because I was attracted by so many others subjects, such as literature, art, philosophy, that I could not make up my mind which to choose. My studies allowed me plenty of leisure. For several years I attended Høffding's seminars. Here I met a group of friends who formed a small circle consisting of 12 members. Hence, the name "Ekliptica". Of their number were the Bohr brothers, the Nørlund brothers, Edgar Rubin, Brøndal and others who later became professors. *Quorum minima pars fui*. I have always associated with those from whom I could learn»[4].

As Skov states that he met the club's members at Høffding's seminars, most part of the historians supposes that Ekliptica started for the purpose of discussing the topics of Høffding's propaedeutic course on philosophy. But such an interpretation is not the only possible because Lis Jacobsen had already passed her examination in propaedeutic philosophy in 1901. Peter Skov in 1902; Astrid Lund in 1903; Niels Bohr, Einar Cohn and N. E. Nørlund in 1904. Harald Bohr, Edgar Rubin, Slomann and Hatt in the summer on 1905 by one of the other two professors of philosophy, Kristian Kroman. Viggo Brøndal passes his examination with Høffding in 1906. Henriksen and P. Nørlund in 1907[5]. Furthermore, there were very close familiar relations among the members: Bohr's brothers and Nørlund's brothers would become related through Niels Bohr's marriage with Margrethe Nørlund, their sister, in 1912. Lis Jacobsen was a Rubin, a cousin of Edgar, so was E. Cohn because his mother was their father's sister. N. E. Nørlund and E. Cohn were class-mates at Søro Academy, a highly prestigious boarding school. Rubin and Slomann took their *studentereksamen* at Slomann's school in 1904. Rubin was also Niels Bohr's half-cousin. Edgar Rubin is unanimously considered the

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[4] PETER SKOV, *Aeres Høst. Erindringer fra mange Lande i urolige Tider* (Harvest of the Years. Recollections from many countries in times of unrest), (Copenhagen: Munksgaard, 1961), p. 10.

[5] FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 25.

Ekliptica founder and he was the one of the group for whom philosophy was the main occupation: when he started his studies at the University he was enrolled in philosophy because an independent course of psychology was established only later on. So the reasons, which brought so many students with different cultural backgrounds together, were their friendship and a common interest in philosophy quite beyond the pleasure to discuss their academic and propaedeutic subjects. In fact, Skov's statement did not refer to Høffding's introductory course, but to his advanced seminars.

In the last session of the interview with Thomas Kuhn and Erik Rüdinger in 1962, Bohr explained that he took a great interest in philosophy in the years after his high school student examination and he came especially in close connection with Høffding. He also added: "at that time I really thought to write something about philosophy […]. I felt that the various problems in psychology, which were called big philosophical problems, of the free will and such things, that one could really reduce them when one considered how one really went about them"[6].

So it is very likely that Bohr attended Høffding's seminars on Kierkegaard, Spinoza's *Ethica*, his lectures on his own philosophy, ethics, theory of knowledge and maybe on Kant's *Kritic der reinen Vernunft*, which Høffding taught in that period according to the Yearbooks of the University, all on subjects which the other members of Ekliptica too must have felt attracted to.

7.2 Harald Høffding as a Mentor

In the Niels Bohr's private library, the Niels Bohr Archive, there is a collection of papers and booklets on philosophical issues, which Høffding sent Bohr with a personal

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[6] NIELS BOHR ARCHIVE, AMERICAN INSTITUTE OF PHYSICS, *Oral History Interview with Niels Bohr*, 11.17.1962, Session V.

dedication and they confirm Bohr's cultural and philosophical orientation within the span of thirty years[7]. It is plausible that Ekliptica was the place where to deepen and compare dialectically one's points of view on various subjects, not least philosophy.

Georg and Naphtali Cohn were law students with a great interest in philosophy, they also attended Høffding's advanced seminars as did the members of Ekliptica, even though it is difficult to say if they joined the club because it seems they were not related to Einar Cohn.

Georg Cohn wrote an article in 1933 that gives a very interesting picture on the atmosphere he felt at Høffding's lectures.

«[…] There was something Socratic about the whole of Høffding's appearance. With the greatest patience and with truly lively interest he listened to the questions of young students and to their early trials in coming to grips with philosophy. He let himself engage in discussions with them about their views and was able to awaken their philosophical interest and personal confidence. The students went to him as one they trusted when subject to doubts and scruples, for instance, with regards to religious questions, about which they may even have avoided talking to their close relatives. Professor Høffding has several times related such incidents and has also written about them in his *Memoirs*. It was at these more intimate gatherings that his talents as a teacher and one able to inspire others truly came to the fore. In his *Memoirs* he tells us that to him it was a source of great happiness to have been a teacher at the University. He appreciated the constant interaction between the study and the lecture hall. He further recalls the fact that

[7] 1) Ved Aarhundredeskiftet (At the turn of the XX Century: the state of philosophy), Det Nordisk Forlag, Copenhagen, 19/12/1900. 2) Danske Filosofer (Danish Philosophers), Udgivet af Universitetsudvalget, 1906. 3) Booklet on "Charles Darwin", Tilskueren, 1909. 4) "Dansk Videnskab (Danish Science), H. Høffding. 5) "David Hume" in Nordisk Tidskrift, Stockolm, 1911. 6) Dissertatio ex Chronici Spinozani, 1921, Die Drei Gedankenmotive Spinozas, (The three motives of Spinoza's thought). 7) Dissertatio ex Chronici Spinozani, 1922, "Das Erste Buch der Ethica" (The first volume on Ethics). 8) Saertryk af Georg og Edv. Brandes (Correspondence between Høffding and Brandes 1870-1923), 1923. 9) Booklet on "Emile Meyerson's Erkenntnistheoretische Arbeiten", 1925. 10) Booklet of Hoffding on "K. F. V. Kroman", 1925. 11) Saertryk af Kvindelige Akademikere (1875-1925) (Academic papers on gender issues). 12) Dissertatio ex Chronici Spinozani, 1927, (On Spinoza). 13) Hans Larsson av Harald Høffding (Särtryck ur Festskrift tillägnad Hans Larsson), Stockolm, 1927. 14) Sociologi og Filosofi (Saertryk Af "Tilskueren") 1927. 15) Saertryk af Gads Danske Magasin, (Blaise Pascal), 1929. 16) "Zur Stellung Der Erkenntnistheorie in Unserrer Zeit", in Kantstudien Philosophische Zeitschrift, Berlin 1930. 17) Saertryk af Nordisk Tidsskrift for Filologi, (Plato and Democritus).

where the students were concerned one generally found oneself touching on or talking about all that which constitutes one's main interest in life – truly the fact of having intercourse with young people at the age when their intellectual and spiritual foundations are being formed – all this makes life rich»[8].

Viggo Brøndal sent Høffding an interesting letter on March 9th 1913 on the occasion of the celebration of Høffding's birthday. The letter gives a further account of the students' devotion for their teacher:

«Dear Professor Høffding, the newspapers announce that you will be celebrating your birthday on Thursday and give an account of all that will be done to honour you and to pay tribute to you on that occasion.

Allow me too, though at present abroad and no longer a member of the group whose custom it was to meet to discuss philosophical questions with you, to send you my heartfelt congratulations.

What makes you the teacher to whom I shall continue to owe most – although the subject I chose was not yours – is the high standards of scholarship and the kind interest you have always shown young students. Such standards have never repelled, nor was your interest ever narrow. One could confide in you about personal matters and projects which were remote from your own. This can be said of very few; there are only few to whom we, as young students, owe a debt of gratitude of such a degree. You have no equal as a teacher for the academic youth of Denmark.

I have no greater wish for you than that you for many years to come may continue to be vitalized by, and find pleasure in, intercourse with the coming generations of students that appear like blossom each year.

Yours sincerely, Viggo Brøndal»[9].

Brøndal's statement "though at present abroad and no longer a member of the group whose custom it was to meet to discuss philosophical questions with you" gives rise to a double interpretation for which either Høffding himself participated in the Ekliptica's meetings or these discussions may have occurred during Høffding's seminars.

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[8] GEORG COHN, "Harald Høffding og hans Filosofi" (Harald Høffding and his Philosophy), *Tilskueren*, 1933, 50: 103-17. From FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 28.

[9] From FAYE, *Niels Bohr* (cit. note 27, ch. 1), p. 29.

Considered the particular dedication from Høffding to his students, that Bohr himself certainly experienced, in addition to Høffding's friendship with Niels's father since the years of his youth, one can infer that Bohr was strongly drawn towards philosophy and here Høffding played a guiding role in directing and encouraging his interests in philosophy. I would like to quote an emblematic passage from the public statement Bohr pronounced on the occasion of Høffding's 85th birthday, on March 11th 1928, published in the newspaper *Berlingske Tidende*: "Høffding has not only guided us into the sublimities of philosophy […], but his unfailing freshness and openness of mind towards every new advance, the development of which, even the most recent, he has kept in every field, has if possible strengthened our confidence in him and won for him the affection of all".

Moreover, we cannot pass over the fact that Bohr chose Harald Høffding as his teacher, while he could attend also Kristian Kroman's propaedeutic course. Even though this last solution would have been unnatural because of Høffding's friendship with Bohr's father.

The preparation of Høffding's examination consisted in a set of books written by Høffding himself: *Kort Oversigt over den nyere Filosofis Historie* (Outline of the history of modern philosophy), published in 1898, *Psykologi i Omrids paa Grundlag af Erfaring* (An outline of psychology on the basis of experience) from 1882, *Formel Logik* (Formal Logic). It was published in 1889, and reprinted in 1889, 1894, 1903, 1907 and 1913. The course lasted two terms, including four hours per week and an oral examination at the end. In the interview with Kuhn quoted earlier, Bohr spoke about an error he had found in Høffding's book on formal logic: "That was just a minor thing, but I pointed out to him that there were some errors, actually there were many errors, in his formal logic. He took that to heart, and there came out a new edition, where he says that he has got some various help from one of his students....". In a new edition Høffding corrected the mistake and in the preface mentioned that one of his former students had observed it. By comparing the two editions, the one from 1903 and the other from 1907 it is possible to make out what the mistake was. In the edition from 1903, Høffding exemplifies the principle of duality or excluded middle and writes that: "a concept (B) must either contain another concept (A) or its negation (a)". Here "either-or" is used in the exclusive sense. Bohr obviously had pointed out that a concept could contain both another concept and this other concepts negation as well. In the edition of the logic from 1907, the principle is stated as follows: "The concept A must either be connected with B". The example given is: "A vertebrate is either a mammal or not a mammal." But, Høffding adds: "In the case where B is not a necessary trait of A both AB and Ab [b being the negation of B] may occur – under different circumstances and at different times. Thus the vertebrate-type can occur as well in mammals as in non-mammals, for instance in the horse as well as in the eagle"[10].

7.3 Edgar Rubin: The Key Person to Understand the Bohr-Høffding Relationship

Edgar Rubin played a pivotal role in the "Ekliptica club" for at least three main reasons: he was the organizer; he studied philosophy and psychology and kept up relations with Høffding.

[10] Cf. FAVRHOLDT, "The Cultural Background of the Young Niels Bohr", *Rivista di Storia della Scienza*, 1985, 2: 445-461, p. 448.

The very first references to the circle omitted two names that were included in the hepta-pointed star indicating seven names and addresses of the remaining members: Gudmund Hatt and Elias Lunding, who became Astrid Lund's husband.

We are interested particularly in Gudmund Hatt and his connections with the club since he fell into oblivion when he went to America just after the foundation of the Ekliptica and many years later he was accused by some members of being sympathetic towards the Germans during the Second World War. Professor Jan Faye found and translated various letters from Rubin and Hatt to Høffding, which confirm the central role that Rubin played among the group's members and Professor Høffding.

On June 28, 1906, Rubin sent Høffding the following letter about Hatt:

«Professor Høffding, from a friend of mine, A.G. Hatt, I have received the enclosed letter which he asks me to pass on to you with a letter testifying to my acquaintance with him. His father is a teacher in Holbæk and is, as far as I understand, a rather peculiar man who has brought Hatt up with an ardent enthusiasm for science. Both the peculiarity and the enthusiasm he has inherited; but his ultimate aim, which is closest to his heart, is to do something in art […]. I became acquainted with him here at the University, when we started as students here at the same time and met each other at the seminars on "Modern philosophers" given by the professor. […]. The people, whom he had spoken to, think, he writes, that an introduction to William James from yourself would prove useful to him. Since I have faith in him and believe that he is made of the right stuff I hope that his request may be granted. As far as I know, Professor Hans Olrik knows him and his family well, and takes an interest in him. Yours sincerely, Edgar Rubin»[11].

From the letter we deduce that Rubin introduced Hatt to Høffding because: 1) Hatt was a friend of him as they met each other at the seminar on "Modern philosophers"; 2) Rubin was in close relations with Høffding as a mentor as it is confirmed by the fact

[11] From FAYE, *Niels Bohr* (cit. note 27, ch. 1), pp. 21-22.

that Professor Høffding accepted Hatt's request, as we can see from a letter from Hatt, dated Dorchester Mass., February 15, 1907[12]. Furthermore, as Hatt was attending both Høffding's series of lectures and seminars in the spring of 1905 (The psychology of free will, Philosophical theories, Lectures on Kierkegaard), Rubin asked him to join the Ekliptica.

Hatt returned to Denmark during the summer of 1907, most probably because of the expenses of studying at Harvard. From another letter to Høffding, dated Holbæk, November 26, 1907, we know that Hatt was once again attending some of Høffding's lectures, even though he had also started studying geography. He then thanks Høffding for the undeserved help he has received, adding that the sympathetic understanding he has met with him has been most helpful and profitable for a young man. Such friendship connected Høffding to several other members of Ekliptica, and in all cases the friendships continued until Høffding's death.

Returning to Rubin (1886-1951), I think he is reputable to be the very key person to prove the special relationship, which connects Bohr to Høffding's philosophy. According to tradition, it was Theodor Fechner who in "Elemente der Psychophysik" (1861) laid the foundations of experimental psychology. For more than fifty years, psychologists believed that mental phenomena consisted of a limited number of elements. Consequently, they considered it their task to discover these elements and the laws relating to their combination or association. Experimental psychologists envisaged these laws as analogous to the laws governing mechanical mixtures or chemical compounds.

[12] *Ibidem*, p. 23.

Edgar Rubin’s teacher, Alfred Lehmann, was even proponent of this experimental trend in psychology. However, during three years (1911-14) of research work in Göttingen under the guidance of the experimental psychologist, G. E. Müller, Rubin became aware of the limits of the analogy between psychology and physical-chemical disciplines.

Rubin became professor of experimental psychology in 1922, after having taught at the University as a reader in philosophy from 1918. He carried on experiments on visual perception, namely on the perception of figures and grounds, which was published in his doctoral dissertation of 1915.

Figure-ground is a type of perceptual organization in vision that involves assignment of edges to regions for purposes of shape determination, determination of depth across an edge and the allocation of visual attention. Figure-ground is a critical process in perception because of its profound consequences for shape perception.

His thesis for the doctorate, “Visuell wahrgenommene Figuren. Studien in psychologischer Analyse” (Visually Experienced Figure. A study of psychological analysis), was one of several treatises, anticipating what in Germany it was called “Gestaltpsychologie”, in France “La psychologie de la forme”. In a lecture “Über Gestaltwahrnehmung”, delivered at “The Eight International Congress of Psychology” (1927), Rubin gave an outline of his psychological methodology, and called his basic view “aspective psychology”.

Rubin’s studies resulted in a new understanding of the conditions under which the same stimuli brought about different states of awareness. The switch in perception from background to figure, and vice versa, and furthermore, the conditions under which different stimuli afforded states of awareness of the same kind in form of colour constancy, constancy of forms, constancy of size, etc. remind us of Høffding’s ideas of totality. Rubin considered the mind as an integrated totality and the various aspects of the mind must be regarded as a whole that cannot be described in isolation.

Rubin acknowledged his cultural debt towards Høffding on various occasions. In the preface to a collection of scientific papers entitled *Experimenta Psychologica*, Rubin cited just Høffding's lectures and seminars and in particular a paper on patriotism he had written many years before, which recalls the topics of Høffding's philosophy:

> «The paper, which comes first, concerns my view on problems similar to those with which my contemporaries among the Gestalt psychologists were occupied. In this connection it was interesting for me to find some notes for a paper on patriotism read in 1905 or 1906 to one of the classes of my teacher, Harald Høffding. Having explained that patriotism is a unitary feeling I said: "With the special name it follows that this feeling is kept separated from the others, but it is just this barrier which I will try to break and show its connection with various other types of feelings. In order to do this we must look at the various aspects of patriotism one after the other. I say "aspects" and not "elements" so that you are not led to believe that patriotism results from a combination of three aspects (or elements). Throughout the following considerations you must keep in view that patriotism is a fact, and that it is within this one fact that I examine the importance of the individual aspects". One will see that the aspective view of wholes, which is advanced in the first paper, can already be traced here»[13].

Rubin makes use of patriotism as an example of the antinomy between analytic and synthetic activity of the mind introduced by Høffding, that is not consistent with the conception of the mind as an integrated totality. Hence, he emphasizes that patriotism is a unitary feeling, not a mere combination of elements. Beyond doubt, Rubin's quotation pertains to Høffding's definition of the nature of consciousness that arises from the well-known *Outlines of Psychology* of 1882:

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[13] EDGAR RUBIN, *Experimenta Psychologica* (Copenhagen: Munksgaard, 1949), p. II.

«We must admit that the idea of self cannot be derived from immediate perception, but must be obtained by inference from the general nature and conditions of conscious life. This is a necessary consequence of the fact that it is based on an activity, always (i.e. so long as consciousness lasts) continued and repeated; on the synthetic activity which all consciousness presupposes. In each individual state we have the product of this activity, but not the activity itself. It is a fact connected with this, that we can never be fully conscious of our self. For the very state in which we think of our self, is conditioned by synthesis; self-consciousness just as every other kind of consciousness, is possible only by its means. The synthesis, the inner unity in us, always hides itself, however deeply we try to penetrate into consciousness; it is the constant presupposition»[14].

Rubin seems to be sceptical about Høffding's conception of synthesis, even if in another passage of the same preface he writes explicitly:

«I might add that already in my first year as a student I was critical of Høffding's "synthesis". Regarding the problems with which I was occupied, the "mental synthesis" of Høffding seemed to me to be a makeshift introduced in order to build up the wholes, which had been broken by "analysis" which led to "sensations". I considered that by not breaking up the wholes and by keeping to them and their aspects in my investigations I could save using this makeshift»[15].

In fact, we have to notice that Høffding, since the *Outlines of Psychology* and still in *The Problems of Philosophy*, affirmed that consciousness and personality "come into being through a perpetual synthesis of elements" although he was aware that "mental synthesis" contradicts the concept that the whole and the parts mutually determine one another:

«Here crops out an antinomy, which is closely connected with the existence of consciousness, and it is peculiar to the concept of personality. Consciousness and personality can, as little, be explained as the

[14] HØFFDING, *Psykologi i Omrids paa Grundlag af Erfaring* (Copenhagen, Gyldendalske Boghandel-Nordisk Forlag, 1882), En. Transl. by Mary E. Lowndes, *Outlines of Psychology* (London: MacMillan, 1904), p. 138.

[15] RUBIN, *Experimenta* (cit. note 13), p. II.

products of previously given elements, as organic life can be explained as the product of unorganic elements. On the other hand, consciousness and personality, just like organic life, come into being through a perpetual synthesis of elements not originally begotten by themselves. It is this *antinomy* which makes the genesis of life and of personality so great a riddle»[16].

Rubin was referring to such an antinomy when he wrote that "mental synthesis" seemed to him a makeshift taken in order "to build up the whole". Rubin confirmed such a position in a lecture on the state of psychology at the end of twenties, published some years later (1956) in "Edgar Rubin *in Memoriam*".

«Firstly, one can have one's doubt in this direction and secondly, it is possible that the victory to a great degree must be regarded as a Pyrrhus victory, even if one disregards this doubt. In a certain way, Høffding was aware of this. Because he understood that there was a peculiarity with his proposed solution. He sustained that when one goes deeper into the relation between the synthesis and the elements, one realizes that there is an antinomy between the synthesis and the elements, which is closely tied up with the essence of the life and sole. For him there was something fundamental and important in this. He was, of course, attracted by the continuous discussion of the problem, but from a more general scientific standpoint, one can perhaps regard a solution that leads to antinomies as a less than a good solution. It seemed to me as something like a smashed vase, which pieces one tried to put together again»[17].

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[16] HØFFDING, *The Problems of Philosophy* (cit. note 28, ch. 1), p.19. Italics are of the present author.

[17] RUBIN, "Bemærkninger angaaende psykologisk Metode", in *Til Minde om Edgar Rubin*, "Nordisk Psykologi Monograph Series", 1956, 8, p. 24. Danish version: «For det første kan man have sine Tvivl i denne Retning, for det andet er det muligt, Sejren i høj Grad er at betragte som en Pyrrhussejr, selvom man ser bort fra denne Tvivl. Paa en vis Maade havde Høffding Forstaaelse heraf. Han forstod nemlig, at der var en Ejendommelighed ved hans formentlige Løsning. Han mener nemlig, at naar man fordyber sig i Forholdet mellem Synthesen og Elementerne, bliver man Klar over, at der her er en Antinomi mellem Synthesen og Elementerne, der nøje hænger sammen med Sjælelivets Væsen. For ham var der noget dybtliggende og betydningsfuldt i dette. Han var jo en Ynder af den stadige Drøftelse af Problemerne, men fra et mere jævnt videnskabeligt Synspunkt kan man maaske betragte en Løsning, der fører til Antinomier, som en mindre heldig Løsning. For mig tog det hele sig ud noget i Retning af, at man først havde en Vase og dernæst slog den i Stykker, hvorefter man, efter at have haft Besværet med dét, prøvede at sæte den sammen igen». The translations from Danish are those of Mrs Felicity Pors (Niels Bohr Archive) in collaboration with the present author.

But, at the same time, Rubin was convinced that “Høffding had held of something central” when he used, as Kierkegaard as his predecessor, “synthesis” in the treatment of those problems that concern the building up of personality[18].

I sustain that Høffding at the time (1882-1902) did not develop yet the concept of wholeness as regards the problems of consciousness and personality as far as he had elaborated his theory of knowledge on the basis of the principle of unity.

As Rubin studied thoroughly Høffding’s philosophy, *he understood* that Høffding had already found the solution to work out the antinomy between synthesis and analysis with respect to psychic experiences: if the conditions of unity and continuity allow describing the phenomena of nature as a unified conception of the whole, the same conditions should permit the psychic experiences to be described in terms of *unity* as regards the formation of personality. In this sense Rubin affirmed, in the foreword to *Experimenta Psychologica*: “Høffding had realized something very important for psychology of perception when he spoke about synthesis”[19]. We have also to recall that Høffding began to elaborate the category of totality since early 1910s, as we deduce from his private correspondence with Emile Meyerson[20].

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[18] Cf. ID., *Experimenta* (cit. note 13), p. II.

[19] *Ibidem*.

[20] FRITHIOF BRANDT, HANS HØFFDING, JEAN ADIGARD DES GAUTRIES, *Correspondance entre Harald Høffding et Emile Meyerson* (Copenhagen: Einar Munskgaard, 1939), pp. 2-3. In particular, we would like to focus on the letter from Høffding to Meyerson, April 16, 1918, in which Høffding gives an interesting clue about the origin of the concept of totality within his philosophy:

«Mon travail philosophique a eu un certain caractère encyclopédique qui est en partie provoqué par les conditions où se trouve la philosophie dans un petit pays comme le mien, où la division du travail ne peut être aussi complète que dans le grands pays. J’ai commencé avec la Psychologie pour passer de là à la Morale et à la Philosophie de la Religion. M’étant ainsi orienté sur des problèmes qui m’avaient intéressé depuis ma première jeunesse, où j’étais sous la profonde influence de Kierkegaard (sur lequel M. Delacroix a écrit un bel article dans la *Revue de Métaphysique et de Morale*), j’ai vu clairemant l’importance de l’Epistémologie pour la méthode et les disciplines philosophiques, et j’ai donné dans mon livre *La Pensée humaine* une recherche sur les fondaments et les relations réciproques des problémes

But when Høffding published *The Problems of Philosophy* in 1902, his view on the theory of knowledge and ontology were in all essential fully developed. Furthermore, many of Høffding's basic suppositions can even be traced back to the early 1880s. Høffding's teaching was not confined to the cultural aspect but became part of his students' life. Rubin expressed publicly his feelings, gratitude and cultural debt once again shortly after Høffding's death.

«Those of us, now advanced in years, who have worked in the field of philosophy and psychology were in close contact with Harald Høffding. He was our teacher, and he influenced us perhaps more than we were aware of. In cases where we developed independent opinion and positions, these came into being due to the fact that, to no negligible extent, we held our ground in the face of his view»[21].

The passage quoted from Rubin's commemorative speech confirms how Høffding influenced his students and how his philosophy was determinant in the development of their scientific speculation.

As it is my purpose to demonstrate how Høffding's teaching may have inspired Bohr's view on reality and his method of scientific inquiry, I will show that Bohr was deeply involved in such philosophical subtleties thanks to his friendship with Edgar Rubin. Since there are no minutes or manuscripts about the Ekliptica's meetings, all we have as regards the details concerning their cultural and philosophical discussions is the correspondence between Bohr and Rubin.

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philosophiques, en insistant particulièrement sur la théorie des catégories. Les livre sur la conception de la totalité est un supplément à la *Pensée humaine*».

We have to notice that the book Høffding is referring to, concerning the conception of totality, was published in 1911 as a supplement to *La Pensée humaine*.

[21] RUBIN ET ALIA, *Harald Høffding in Memoriam. Fire taler holdt paa Københavns Universitet paa Harald Høffdings 89 Aars Dag 11. Marts 1932*, (Four speeches made at the University of Copenhagen on Harald Høffding's 89th Birthday March 11, 1932), (Copenhagen: Gyldendal, 1932).

In the Niels Bohr Archive there are letters concerning the private correspondence between the two authors in the period 1909-1955, out of which the more interesting are those written between 1912 and 1915 when Bohr was in Manchester. As we know, that period was very crucial for Bohr's scientific production: he was writing his "Rutherford Memorandum" which would lead him to the famous trilogy "On the Constitution of Atoms and Molecules". Bohr has travelled a very long way from the electron theory of metals of 1911, when he defended his doctoral dissertation. He has chosen an atom model, justified its use, and selected a quantum condition to ensure its stability. In the Memorandum he immediately proceeded to investigate its power in application. There is a letter, dated May 15, 1912, in which Rubin thanks Bohr for a postcard. Bohr was very busy and, in the card, he regrets not having enough time to write. As in other letters, Rubin utilizes his correspondence with Bohr to elucidate him about his studies by using samples of theories in order to explain general concepts of common language: "As an example I can mention an investigation of retroactive restriction. When one learns a series of meaningless letters "by heart", it appears that one at a later stage can repeat many fewer, when one just after the learning has started on difficult mental work than when one has just relaxed. When one now – instead of requiring repetition, say a series of old and new letters – asks the test person to say which he recognises and those he doesn't, it turns out that the retroactive restriction caused by difficult mental work has no effect on recognition"[22]. Just a few days later Bohr answered Rubin. The letter is dated May 20:

[22] NIELS BOHR ARCHIVE, *Bohr Private Correspondence*. Danish version: «Som et lille eks kan jeg nævne en undersøgelse over tilbagevirkende hæmning, han i denne tid har ladet udføre. Naar man lærer en række meningsløse stavelser udenad, saa viser det sig, at man senere kan reproducere langt færre, naar man lige efter indprægningen har givet sig af med anspændt aandeligt arbejde, end naar man har holdt sig i ro. Naar man nu – stedet for at forlange reproduktion – forlanger, at forsøgspersonen, i det man fremfører en

«Dear Edgar, forgive me for disturbing you once again with my nonsense; I do not at all have time for it myself either; but my mind will not be at rest until I have asked you whether it makes any sense at all to say something like this: recognition (which according to its nature is purely qualitative; and where there is a new external impression to break down inhibitions) depends primarily only on the state at the 2 points in time, and only secondarily on the state in between (via conditioned or natural effacement); whilst reproduction (which perhaps is more quantitative (selective) ??? and where inhibitions must be broken down "from within") primarily depends both on the state at the point in time of the impression and on inhibitions brought about by the state at the moment of reproduction and the time in between. (Reproduction can, of course, be facilitated by stimulants such as alcohol or hypnosis (in such cases where recognition would take place???). Dear Edgar, you must not think that I do not understand that such things only can be said with a chance of it being correct or coherent after a veritable study and critique or that I have become a wild and raving dialectician […]»[23].

The letter testifies once again Bohr's involvement in Rubin's studies as they were used to discuss about such arguments from time to time. Hence, Bohr admits "the truth is rather that it is such a long time since I have had the opportunity to speak about such things […]".

Another letter from Rubin to Bohr concerning philosophical arguments is dated September 30, 1915; it contains a reference to the Kantian antithesis between the phenomena (the reflected image) and the thing in itself (the reflected object).

«The similarity between the reflected image and the reflected object can certainly be very great, but on the other hand the difference between our conduct, when we have to do with reality, or a reflected image is very great.

It is interesting to realize how apparently futile are the very different criteria we utilize, and the difference between their particular nature and the abstract "correlations" one usually applies in such cases.

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række nye og gamle stavelser, skal meddele hvilke han genkender og hvilke ikke, saa viser det sig, at den fra det anspændte aandeligt arbejde udgaaende tilbagevirkende hemning slet ikke har nogen indflydelse paa genkendelsen».

[23] FAVRHOLDT (ed.), *NBCW*, 10 (cit. note 25, ch. 6), p. 575.

It is also very interesting how these criteria can work quite causally, so we do not realize at all from where we know that we are dealing with a reflected image or as a logical basis; there is here an area where it is very difficult to keep logical basis and cause apart from each other separated»[24].

The passage above seems also to introduce the need to deepen the debate on sciences' foundation. Furthermore, Rubin depicts a sort of complementary view when he talks about the difficulty of keeping "logical basis and cause apart from each other separated".

There is a common line of thought that emerges from Rubin, Høffding and Bohr's conceptions with respect to psychology: that it is not possible to regard ideas/ thought as isolated parts.

Professor David Favrholdt points out that Bohr's concern as regards psychology was the conditions for the description of mental events and/or the description of the brain processes corresponding to them. He also mentions that such a thesis contradicts Høffding's view presented in the *Outlines of Psychology*, namely the conception of "association-psychology". Apparently, Favrholdt is unaware[25] that Høffding, in *The Problems of Philosophy* of 1902, already had distanced himself from such a conception: "There is always a tendency – the livelier the consciousness, the stronger the tendency – toward a rounding out or widening, by means of which the single ideas enter into

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[24] NIELS BOHR ARCHIVE, *Bohr Private Correspondence*. Danish version: «Ligheden mellenr Spejlbillede og det afspejlede kan jo vaere meget stor, man paa den anden Side en Forskellen i vor opførsel om det er med Virkeligheden eller et Spejlbillede vi har at gøre meget stor. Det er interessant at gøre sig klart hvor tilsyneladende intetsigende de højst forskelliger Kriteria vi anvender er og hvor langt der er mellem deres specielle Karacter og det abstracte Kriterium "Sammenhaeng" man plejer at anføre i saadenne Tilfelle. Det er ogsaa ganske interessant hvorledes disse Kriteriar kan virke enten ganske kausalt saa vi slet ikke gør as klart hvorfra vi ved at vi har med et Spejlbillede at gøre elen som en logisk Begrundelse; der en her et Omraade hvar det er meget vanskeligt at hvold logisk Ground og Aarsag ude fra hinanden».

[25] FAVRHOLDT (ed.), *NBCW*, 10 (cit. note 25, ch. 6), p. XLIV: «This was the view of the associationist psychologists at the end of the 19th century and the view Høffding presented in his *Outlines of Psychology*». Favrholdt did not mention, in any of his writings, Høffding's critical positions with regard to association psychology.

combination with other ideas according to fixed laws. The so-called association-psychology (among whose adherents I have sometimes been unjustly numbered) conceives the single ideas as independent atoms, which in a purely external, mechanical fashion are brought into combination"[26].

Høffding recognized that in the process of association it is the connected whole that exercises its power over the single ideas. Høffding was aware, as Rubin and Bohr too, that the ideas never appear in a complete isolation in conformity with the concept of unity and the category of totality.

In 1961 Bohr wrote an important article on the unity of human knowledge that seems to be his cultural testament. The paper draws the ideal line between Bohr's early interests in philosophy since the years of the Ekliptica's gatherings and the concept of complementarity developed later on.

«In particular, the conditions of analysis and synthesis of so-called psychic experiences have always been an important problem in philosophy. It is evident that words like thoughts and sentiments, referring to mutually exclusive experiences, have been used in a typical complementary manner since the very origin of language. In this context, however, the subject-object separation demands special attention. Every unambiguous communication about the state and activity of our mind implies, of course, a separation between the content of our consciousness and the background loosely referred to as "our self", but any attempt at exhaustive description of the richness of conscious life demands in various situations a different placing of the section between subject and object»[27].

In the last quotation Bohr starts from the conditions of analysis and synthesis of psychic experiences to affirm that different mental activities exclude others in a complementary way. In this like in other philosophical papers, Bohr extends, in an analogical or

[26] HØFFDING, *The Problems of Philosophy* (cit. note 28, ch. 1), p. 19.

[27] BOHR, "The Unity of Human Knowledge" (cit. note 24, ch. 1), p. 16.

metaphorical way, the physical notion of complementarity to other fields of human knowledge (psychology, biology, political science). As far as the psychological field is concerned, it is Bohr's suggestion that thought and feeling seem to exclude each other. Namely, emotional aspects are excluded by very deep concentration of thought that, vice versa, is excluded by great emotional excitement. One can find a similar complementarity in perception: the emotional experience of a piece of music excludes conscious analysis of it, whereas analysis of the music excludes emotional experience. Both approaches are necessary for our understanding of what music is[28]. Another example is the perception of ambiguous pictures. It is not surprising that in Rubin's doctoral thesis defended in 1915 was presented the famous figure, "Rubin's vase", which may be perceived as a vase or as two profiles, the one perception excluding the other. Bohr and Rubin independently of one another developed similar conceptions of complementarity in different fields of study. As Bohr never acknowledged Rubin's precedence as regards complementarity, there must be some "genetic" connection between the two conceptions for giving account of such a striking similarity. Høffding's theory of knowledge, based on the concept of unity and continuity and the category of totality, is the key to decipher this common "genetic matrix". Rubin's role is essential to support the claim of Bohr's debt towards Høffding. In fact, as we have seen, Rubin developed his "aspective psychology" following Høffding's theory of knowledge, which Bohr himself was well aware of.

[28] Cf. FAVRHOLDT (ed.), *NBCW*, 10 (cit. note 25, ch. 6), p. XLVI.

Moreover, the legacy of Høffding's teaching in Bohr's mind took the form of a deep comprehension of the unity and harmony of nature such as of "the unity in all human searching for knowledge"[29].

[29] Cf. ROZENTAL, "Childhood and Youth" (cit. note 2, ch. 6), p. 13.

Chapter 8

Atomic Models and Quantum Hypothesis

8.1 At the Origin of Bohr's First Atomic Model

Niels Bohr's first atomic model was presented in 1913 in a long article published in three parts in volume 26 of the *Philosophical Magazine*. The article, titled "On the Constitution of Atoms and Molecules", provided theoretical foundations for the Rutherford atomic model of 1911. Bohr's approach adopted in the paper was considered as original because of the successful use of quantum concepts in the solution of problems concerning the constitution and physical problems of the atoms. As a matter of fact, until 1910, most part of physicists (with the exceptions of Einstein, Von Laue and Ehrenfest) was convinced that Planck's constant *h* was characteristic only of the problem of heat radiation. Therefore, Bohr's work assumed a two-fold importance in the history of 20th century physics. On the one hand it would represent the first attempt to elaborate a consistent theory on the constitution of the atom, on the other hand, it would stand as a decisive progress of quantum conceptions by establishing their high level of generality. Nevertheless, Bohr's attempt concretized itself in a research program having the purpose of expanding the knowledge of atomic phenomena, and, at the same time, of revising the foundations of physics. This research program, as it has been noted, lasted until 1927, when Bohr himself provided a physical interpretation of quantum mechanics based on the principle of complementarity.

As Professor Petruccioli noted, Bohr's paper of 1913 is situated in a historical perspective extending far beyond the role it played on its first appearance with regard to the internal questions of a certain domain of physics, i.e. those concerning the nature and the observable behaviour of microscopic objects. Only in this broader context is it possible to reconstruct the complex network of conceptual and methodological relations stretching from the appearance of Planck's first quantum hypothesis to the establishment of the new mechanics during 1920s. In fact, as Bohr later pointed out, some 30 years of research were needed to unveil the real physical significance and bring out all the conceptual implications of Planck's idea, according to which the quantity of energy exchanged between a field of radiation and an oscillator of frequency ν (a charge oscillating around a position of equilibrium) is not the result of a continuous process but rather one dependent on distinct events, each involving a finite and indivisible quantity of energy. This interpretation of Planck's ideas was to remain controversial for many years, especially with regard to the inevitable, disastrous consequences for classical electrodynamics supposedly deriving from the concept of discontinuity. Planck's original formulation of black-body theory had, in fact, employed quantization as a hypothesis regarding the way in which the total energy of a system may be distributed among N oscillators of differing frequency. That is, it was used as a basis for certain probability calculations required to obtain the value of the entropy of the system and, at least until 1908, was not regarded by Planck as interpretable from a physical point of view as an application of the concept of discontinuity to atomic processes. Moreover, it did not was a process lying beyond the interpretative scope of 19th century electrodynamics, i.e. a non-classical process. The conviction that the quantum should be associated with the idea of discontinuity was to take gradual hold only later, even after the first demonstrations that the quantization of energy was applicable to other sectors of experience (e.g. in Einstein's interpretation of the photoelectric effect)[1].

As a result, the quantum *h* acquired a far deeper physical meaning than Planck originally wanted to assign to it[2].

Einstein's theory of relativity and Planck's quantum theory emphasized the limits of the foundations of classical physics so to encourage the scientific community to revise the whole sector of science. With such a scientific background, Bohr's attempt to elaborate the quantized atomic model would appear justifiable and rational legitimate. Namely, the acceptance of the Rutherford model composed of a positively charged nucleus and a number of electrons moving freely in predetermined orbits; and the recognition of the limitations of classical electrodynamics in accounting for the radioactive stability of electrons; and finally the application of Planck's hypothesis to the model, which would give rise to the following assertions:

1. The electrons can only occupy discrete orbits to which is associated a value of energy derivable from the so-called quantum condition for stationary states.

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[1] Cf. PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), pp. 37-38.

[2] «Even when Einstein's reputation grew, as it quickly did, his views on the necessity of the quantum discontinuity remained suspect because they were repeatedly coupled with the generally rejected light-quantum hypothesis. If the physics profession was to recognize the challenge of Planck's law, better established figures would need to be persuaded that it demanded a break with classical physics. In the event, several of them quickly were. During 1908 Lorentz produced a new and especially convincing derivation of the Rayleigh-Jeans law. Shortly thereafter he was persuaded that his results require his embracing Planck's theory, including discontinuity or some equivalent departure from tradition. Wien and Planck quickly adopted similar positions, the former probably and the latter surely under Lorentz's influence. […] These are the central events through which the energy quantum and discontinuity came to challenge the physics profession». From KUHN, *Black-Body Theory and the Quantum Discontinuity, 1894-1912* (New York: Oxford University Press, 1978), p. 189. This Kuhn's thesis has been object of criticism. In particular, Martin Klein ("Paradigm Lost? A Review Symposium, *Isis*, 1979, 70: 429-34) challenged the historiographical accuracy of one of the main theses of Kuhn's work, i.e. that Planck's original derivation of the law of distribution of radiation is firmly anchored within the classical tradition and that the notion of discontinuity plays no role therein. It is, for that matter, known that Kuhn himself described the work as a historiographical heresy.

2. The atom emits and absorbs energy in the form of radiation not continuously but in quanta – to each process of radiation emission there corresponds the transition of an electron from one stationary state to another, from one orbit to another of different value of energy.

Nevertheless, as it will be shown, the originality of Bohr's work does not boil down to the application of Planck's hypothesis to the problem of atomic constitution. As a matter of fact, in this as in other cases, the development of knowledge is not characterized by phases of sudden discontinuity; such steps were consequences of the concrete problems with which Bohr was dealing with.

«This seems to be nothing else than what was to be expected, as it seems to be rigorously proved that the mechanics is not able to explain the experimental facts in problems dealing with single atoms. In analogy to what is known for other problems it seems however to be legitimate to use the mechanics in the investigation of the behaviour of a system, if we only look apart from questions of stability»[3].

Bohr motivated his recourse to the hypothesis by explaining that the ratio between the kinetic energy and the frequency of an atomic electron always has a definite value. This was the first attempt at the quantization of the model of the atom and the hypothesis is contained in a memorandum delivered to Ernest Rutherford in the summer of 1912 (between June and July) at the end of his studies in England, in which Bohr outlined the plan of a subsequent paper on molecular structure and stability. In his view, this was a solution that seemed "to offer a possibility of an explanation of the whole group of experimental results" and to confirm, to some extent, Planck and Einstein's conceptions

[3] Cf. "Rutherford Memorandum", in ROSENFELD, HOYER (eds.), *NBCW*, 2 (cit. note 34, ch. 2), pp. 136-58. The quotation is taken from the note on p. A2 of the manuscript.

of the mechanism of radiation[4]. It is worth remarking that there was nothing really new in the conviction that the answer to the many problems encountered in the study of matter and involving direct reference to the atom's internal constitution were to be found in some non-mechanical hypothesis. As it shall be shown in the chapters to follow, what was really new in Bohr was the way in which he dealt with classical physics in the study of the new sector of phenomena by using a new hypothesis. Other physicists had proposed to use non mechanical hypothesis without success, and Bohr had himself been gathering significance evidence in this sense ever since his doctorate thesis (1911) on the possible developments and the degree of generality of the electronic theory of metals formulated by Lorentz in 1905. In particular, while Bohr recognized that Lorentz' theory was mathematically perfect, he suggested that the physical assumptions on which it was based was not valid. The aim of Bohr thesis was to attempt to carry out the calculations for the various phenomena that were explained by the presence of free electrons in metals in as great generality as possible. In fact, Bohr tried to extend Lorentz' theory to explain problems such as heat radiation and magnetism by assuming:

1. that free electrons are present in any piece of metal, their number depending on the nature and temperature of the metal, but their kind being the same in all metals.
2. That mechanical heat equilibrium will exists between the free electrons and the atoms in a homogeneous piece of metal of uniform temperature and not subjected to external forces.

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[4] *Ibidem*.

3. In addition to the above general assumptions, he supposed that the properties of the individual atoms of the metal are, on the average, the same in all directions, and that this *isotrony* will remain, independently of the presence of external forces[5].

Thus, Bohr was able to show that the electronic theory of metals was not a solid basis to derive the explanation of the fundamental properties of matter from the conceptions of classical physics. In more general terms, it was demonstrated the insufficiency of electromagnetism to express the real physical nature of microscopic objects.

8.2 The English Period

In the autumn of 1911 Bohr arrived in Cambridge, where he vainly tried to bring his ideas on the electron theory of metals to John Joseph Thomson's attention[6]. Moreover, The Cambridge Philosophical Society found the English version of his thesis too long to be published[7]. It was in the middle of March 1912 that Bohr moved to Manchester, where, working at Rutherford's laboratory, he found the congenial atmosphere his sensitive nature required. In this period he published a paper on the decrease of velocity

[5] Cf. BOHR, *Studier Over Metallernes Elektrontheori* (Copenhagen: PhD Dissertation, 1911); in ROSENFELD, RUD NIELSEN (eds.), *NBCW*, 1 (cit. note 17, ch. 1), pp. 299-300. Bohr officially presented his thesis on May 13, 1911 and discussed it with mathematician P. Heegaard and physicist C. Christiansen.

[6] In a long letter to his brother Harald dated October 23, 1911, Bohr spoke of the difficulties he had encountered in interesting Thomson in his work: «In fact, Thomson has not so far been as easy to deal with as I thought the first day. He i san excellent man, incredibly clever and full of imagination (you should hear one of his elementary lectures) and extremely friendly; but he is so immensely busy with so many things, and he is so absorbed in his work, that it is very difficult to get to talk to him. He has not yet had time to read my paper, and I do not know if he will accept my criticism». Nevertheless, from their brief conversations Bohr got the impression that Thomson did not agree with the conclusions of his work: «[…] he thinks that a mechanical model can be found which will explain the law of heat radiation on the basis of the ordinary laws of electromagnetism, something that obviously is impossible, as I have shown indirectly […]». From *NBCW*, 1 (cit. note 17, ch. 1), p. 527.

[7] With regard to the problems encountered in publishing his doctorate thesis, Bohr informed his brother that he had had discouraging news from Larmor about the prospects of his request to the Royal Society: «[…] he thinks it will be impossible, not because it has been published in Danish, but because it contains criticism of the work of others, and the Royal Society considers it an inviolable rule not to accept criticism that does not originate in its own publications». From *NBCW*, 1 (cit. note 17, ch. 1), p. 529.

of charged particles on passing through matter, from which he obtained further hints for his approach to the problems of atomic structure[8].

The studies carried on during these months were at the origin of the so-called memorandum Rutherford, where Bohr indicated what should constitute the methodological basis for the rational foundation of atomic physics:

1. The use of classical mechanics to study the behaviour of atomic systems.
2. The necessary choice of hypotheses conflicting with the classical mechanics.

It is exactly the methodological path that Bohr decided to follow, starting from the above epistemologically risky assumption, which will be interesting to shed light on. Although, at the same time, Bohr himself regarded this assumption as imposed by empirical data and by the more probable atomic structure deriving from it.

The study of the structure of the atoms was a new sector of research in physics. In fact, only after the turn of the century the idea became central in physical research that atoms were complex physical objects endowed with an internal structure. In 1897, J. J. Thomson's measurements provided empirical justification for the hypothesis that electrons were present within atoms and that they were situated in a state of elastic linkage forming a system of oscillating charges or charges rotating on particular orbits. In December 1903 Thomson proposed a model in which "the atoms of the elements consist of a number of negatively electrified corpuscles enclosed in a sphere of uniform positive electrification"[9].

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[8] BOHR, "On the Theory of the Decrease of Velocity of Moving Electrified Particles on Passing through Matter", *Philosophical Magazine*, 1912, 25: 10-31. Reprinted in *NBCW*, 2 (cit. note 34, ch. 2), pp. 18-39.

[9] JOHN J. THOMSON, "On the Structure of the Atom – An Investigation of the Stability and Periods of Oscillation of a Number of Corpuscles Arranged at Equal Intervals around the Circumference of a Circle;

The discovery of the atomic nucleus, as is known, may be dated from 1910, when Rutherford was pondering over the unexpected occurrence of large angle scattering of α-particles from metal plates. In the famous paper of 1911 on the scattering of α and β-particles by matter and the structure of the atom, Rutherford quoted an article of 1904 by the Japanese physicist Hantaro Nagaoka[10].

In this study[11] Nagaoka considered under the name of "Saturnian system" a model of the atom consisting of a positively charged central particle surrounded by rings of electron rotating with a common angular velocity. Nagaoka's concern was to account both for the optical spectra and the radioactive emission of β-particles by heavy elements. He tried to ascribe the origin of the spectral lines to oscillations of stable ring configurations without knowing that J. Perrin had anticipated him yet in 1901, when he suggested that a "structure nucléo-planétaire" of the atom would provide room both for radioactive instability and for stable configurations of different chemical velocities. Nagaoka's attempt at interpreting spectral lines on the basis of his "Saturnian model" was taken up and elaborated by J. W. Nicholson in a series of papers between November 1911 and September 1912. Nicholson was interested in the unidentified lines appearing in nebular spectra and in the spectra of the solar corona, although no relation

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With Application of the Results to the Theory of Atomic Structure", *Philosophical Magazine*, 1904, 7: 237-65.

10 ERNEST RUTHERFORD, "The Scattering of α and β-Particles by Matter and the Structure of the Atom", *Proceedings of the Manchester Literary and Philosophical Society*, 1911, 55: 18-20 (abstract of the paper). And "The Scattering of α and β-Particles by Matter and the Structure of the Atom", *Philosophical Magazine*, 1911, 21: 669-88 (paper). The articles have been reprinted in *The Collected Papers of Lord Rutherford of Nelson*, 3 Vols, Vol. 2 (New York: Interscience, 1962), 212-13, 238-54.

11 HANTARO NAGAOKA, "Kinetics of a System of Particles Illustrating the Line and the Band Spectrum and the Phenomena of Radioactivity", *Philosophical Magazine*, 1904, 7: 445-455.

can be traced between his work and Rutherford's discovery[12]. Rutherford arrived at the following conclusion: "In order to explain these and other results, it is necessary to assume that the electrified particle passes through an intense electric field within the atom. The scattering of the electrified particles is considered for a type of atom, which consists of a central electric charge concentrated at a point and surrounded by a uniform spherical distribution of opposite electricity equal in amount"[13]. On the basis of this model, Rutherford obtained the formula for the scattering of α-particles and the angular dependence of the distribution observed. It is worth noticing that, at the time, Thomson's model was still the reference point for those physicists who tried to determine the relation between the number of electrons and the atomic weights of the elements and to provide an explanation of their physical and chemical properties in relation to their position in the periodic table. For this reason Rutherford's new model was not seen as self-evident as one would now expect.

Bohr's arrival in Manchester after his period in Cambridge was preceded by great expectations as he could give important contributions to the researches inaugurated by the new model. As is known, the problem of the orbital instability was the weak point of the model. However, this question did not preoccupy Bohr, given his conviction that this was a symptom of the already known impossibility of treating the investigation of atomic structure with classical tools. For this reason the procedure of quantization was not aimed at providing a consistent solution to this problem, but it was a consequence of the impossibility of describing the orbital motion of charged particles with the Maxwell-Lorentz laws of electrodynamics.

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[12] Cf. ROSENFELD, "Introduction" to BOHR, *On the Constitution of Atoms and Molecules* (Copenhagen: Munksgaard, 1963), pp. XI-XII.

[13] RUTHERFORD, "The Scattering" (cit. note 10), pp. 18-20.

As it was noted, in those years the question of radioactive instability was, unlike that of mechanical instability, not a criterion discriminating between one picture of the atom and another, given that the same instability affected any model containing electronic charges in motion[14].

It should be pointed out that Planck's concept of the quantum of action was not a simple makeshift to work out the problem of the stability, but rather a sign of Bohr's conviction that physics had moved definitely beyond the scope of classical theory and that a new system of conceptual reference was therefore required[15].

As Petruccioli noted, the stability that Bohr sought to restore to Rutherford's model of the atom so as to demonstrate its definitive superiority to Thomson's is closely connected with a difficulty encountered in the application of mechanical considerations to the study of the electron configuration: knowledge of the intensity of the central charge and of the number of electrons contained in a ring provides no useful information as to the frequencies of movement of the electrons themselves. In fact, all that can be derived from such considerations is the relation $(e^2/a^2)X = ma(2\pi\nu)^2$ between the frequency ν of revolution and the radius a of the ring, which says nothing about the possible states of oscillation of the electronic particles since with the varying of an infinitely great number of frequencies may be obtained[16].

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[14] HEILBRON and KUHN (cit. note 43, ch. 2, p. 241) have stressed the error often found in the literature and manuals arising from the attribution of a crucial role in the development of the first theory of the atom to radioactive instability. Against such views [e.g. ROSENFELD, "Introduction" (cit. note 12)] they object that "radioactive, unlike mechanical, instability does not distinguish Rutherford's atom from Thomson's" and that "the problem of radioactive instability was well known and seems to have caused little concern".

[15] This is the view expressed by Bohr himself, see *infra*.

[16] PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), p. 43.

Given this state of things, it was clear that only a non-mechanical hypothesis would be able to account for the stability characteristic of atoms existing in nature and to suggest a rigorous criterion to distinguish out of all the mechanically possible states those physically admissible. The idea that Bohr put forward in the memorandum was that the latter satisfied the condition whereby the ratio between an electron's kinetic energy and its frequency assumes a definite value, which he posited as equal to constant *K*. It is interesting to note that in the memorandum the formulation of the *K* hypothesis made no explicit mention of the quantum of action. Whereas Rosenfeld sustained that there is no doubt that Bohr had fully realized that the stabilizing element of atomic structures on the Rutherford model had to be sought in the quantum of action, Petruccioli pointed out that little remained in this condition of Planck and Einstein's quantum hypotheses. In fact, in this case not only had any procedure for the quantization of the oscillator's total energy disappeared to be replaced by a simple dependence of frequency on kinetic energy, but the constant of proportionality appearing in the latter relation was by no means reducible to some multiple or significant sub-multiple of Planck's quantum of action. There is evidence to believe that when Bohr wrote the memorandum he was still in the dark about the value of *K*, and that he did not make any progress on this point until he attacked the problem of the hydrogen spectrum. As Rosenfeld pointed out, the numerical value $1.3 \cdot 10^{-11}$ erg, ascribed at the end of the memorandum to the quantity denoted by *A*, is different from that which would correspond to the choice $K = 1/2\ h$. For the quantity *A* may be identified with $W_0\ (h/2K)^2$, where W_0 denotes the atomic energy unit $2\pi^2 e^4 m/h^2$, numerically equal to $2.0 \cdot 10^{-11}$ erg; and the estimated value for *K* is about $0.6\ h$[17].

[17] ROSENFELD, "Introduction" (cit. note 12), pp. XXX-XXXI.

Despite these conceptual and formal obstacles, Bohr was convinced that he had found out something about the structure of atoms and managed to get hold of a little bit of reality[18].

On March 6, 1913 Bohr sent Rutherford the first part of the paper on the constitution of atoms and molecules. Bohr's standpoint on the whole question showed a profound change from the solutions sketched out in the memorandum. The procedure of quantization was different and there appeared a problem which had been overlooked in the earlier memorandum and which entailed the redefinition of the theory's very objectives: "I have tried to show that from such a point of view it seems possible to give a simple interpretation of the law of the spectrum of hydrogen, and that the calculation affords a close quantitative agreement with experiment"[19]. This change was influenced by comparison with Nicholson's theory, which suggested alternative solutions for the quantization of a Rutherford's model of the atom and by Bohr's "discovery" of the empirical laws of atomic spectroscopy[20]. In particular, as Bohr wrote in a letter to Rutherford dated January 31st 1913, he had been struck by some articles of the astrophysicist Nicholson:

> «In his calculations, Nicholson deals, as I, with systems of the same constitution as your atom-model; and in determining the dimensions and the energy of the systems he, as I, seeks a basis in the relation between the energy and the frequency suggested by Planck's theory of radiation»[21].

Nevertheless, Nicholson's reference to Planck remained only a vague analogy, as he obtained for the ratio between the electron's total energy and their frequency a series of

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[18] Letter from Niels to Harald Bohr, June 19, 1912, in *NBCW*, 1 (cit. note 17, ch. 1), p. 559.

[19] Letter from Bohr to Rutherford, March 6, 1913, in *NBCW*, 2 (cit. note 34, ch. 2), pp. 581-83.

[20] See *infra* Bohr's discovery of Johann Balmer's formula.

[21] Letter from Bohr to Rutherford, January 31, 1913, in *NBCW*, 2 (cit. note 34, ch. 2), pp. 579-80.

values approximating closely to whole multiples of the constant *h*. As it will be shown below, Bohr pointed out to Rutherford that the differences between the two theories were justified by the fact that Nicholson was mainly concerned with the spectra emitted by certain gases present in the solar corona and hence with highly unstable states of the atom in which an element may emit radiant energy.

He concluded:

«I must however remark that the considerations [regarding Nicholson's theory] play no essential part of the investigation in my paper. I do not at all deal with the question of calculation of the frequencies corresponding to the lines in the visible spectrum. I have only tried, on the basis of the simple hypothesis, which I used from the beginning, to discuss the constitution of the atoms and molecules in their permanent state»[22].

However, such as Bohr was aware, the weak point of Nicholson's theory coincided with the core of his revolutionary proposal that for reasons of its own internal consistency required the search for solutions, which would justify the simultaneous use of the tools of analysis and description provided by classical mechanics and quantum-theoretical hypotheses.

This was Rutherford's reaction:

«Your ideas […] are very ingenious and seem to work out very well: but the mixture of Planck's ideas with the old mechanics makes it very difficult to form a physical idea of what is at the basis of it all. There appears to me one grave difficulty in your hypothesis, which I have no doubt that you fully realise, namely, how does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you have to assume that the electron knows beforehand where it is going to stop»[23].

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[22] *Ibidem*.

[23] Letter from Rutherford to Bohr, March 20, 1913, in *NBCW*, 2 (cit note 34, ch. 2), pp. 583-84.

Even Rutherford, as is evident, grasped the weak point of Bohr's proposal that imposed to operate in the context of classical approach and, at the same time, the theory needed hypotheses, which violated common sense and imposed a limit to the formation of classical ideas: as it was for the electrons which decided and knew beforehand the evolution of the process.

Chapter 9

From Bohr's *Trilogy* to "On the Spectrum of Hydrogen"

9.1 The Foundation of a Research Program

The first part of the trilogy "On the Constitution of Atoms and Molecules" opens with an introduction in which Bohr recalled the difference between Thomson's model and Rutherford's as regards the mechanical stability of the orbits.

«In the attempt to explain some of the properties of matter on the basis of this atom-model we meet, however, with difficulties of a serious nature arising from the apparent instability of the system of electrons: difficulties purposely avoided in atom-models previously considered, for instance, in the one proposed by Sir J. J. Thomson […]»[1].

Soon afterwards, Bohr made the point on the development of the theory of the energy radiation in light of the most recent experiments as regards different phenomena such as specific heats, photoelectric effect, Röntgen rays. Furthermore, he supported such considerations with a reference to the proceedings of the first Solvay Conference held in Brussels in November 1911, where the subject of the discussion was just the different procedures of quantization. In fact, Arthur Erich Haas had carried out yet one of the

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[1] BOHR, "On the Constitution" (cit. note 34, ch. 2), pp. 1-2.

main attempts at the quantization of the atom's model in 1910. Arnold Sommerfeld during the Solvay Conference referred explicitly to Haas's hypothesis and stressed the importance of the existence of some connection between the constant *h* and the atom's dimensions.

Therefore the general state of the research suggested Bohr to modify the approach to the problem of the model's stability through the introduction of a quantity foreign to classical electrodynamics such as the quantum of action.

It is worth noticing that the use of the quantum of action was regarded as required by the new theoretic approach independently of the equations of motion of the electrons, which could be derived from either model. Therefore, the question of the mechanical stability of the orbits had no importance in the choice of atomic model, as on the contrary had it at the time of the Memorandum Rutherford. As Bohr himself noted:

«By the introduction of this quantity the question of the stable configuration of the electrons in the atoms is essentially changed, as this constant is of such dimensions and magnitude that it, together with the mass and the charged of the particles, can determine a length of the order of magnitude required»[2].

The above mentioned statement is full of consequences as the introduction of the constant *h* can help to work out the weakest point of Rutherford's model: the impossibility of determining the order of magnitude of the atom's linear dimensions. In fact, by the comparison of Thomson's model with Rutherford's, the latter do not allow the obtainment of the dimensions of the system through simple mechanical calculations.

Following these premises, Bohr introduces the objectives he intended to achieve with his work:

[2] *Ibidem*, p. 2.

«The paper is an attempt to show that the application of the above ideas to Rutherford's atom-model affords a basis for a theory of the constitution of atoms. It will further be shown that from this theory we are led to a theory of the constitution of molecules.

In the present first part of the paper the mechanism of the binding of electrons by a positive nucleus is discussed in relation to Planck's theory. It will be shown that it is possible from the point of view taken to account in a simple way for the law of the line spectrum of hydrogen»[3].

We can also synthesize the goals of Bohr's program as follows:

- It is possible to construct a consistent theory of the atom's constitution by application of quantum ideas to Rutherford's model.
- This theory can be seen as a generalization of Planck's theory and permits to illustrate the mechanism binding the electrons around the nucleus.
- The theory should offer an interpretation of the line spectrum of the hydrogen atom as the Balmer's formula may be derived from the theory itself.

We have to remind ourselves that the general theoretical problem of the program was to find an acceptable theoretical explanation of the atom's stability. What did induce Bohr for the time being to derogate from such a priority?

Among historians of science the idea is widespread that Bohr's program was based on some conceptual assumptions and methodological precepts. These principles are not backed by unquestionable results but they arise from judgements or rather personal convictions as to the potential for development possessed by classical conceptions and conceptual implications of quantum physics[4]. Bohr was aware that classical electrodynamics was not applicable to the description of the behaviour of atomic

[3] *Ibid*.

[4] PETRUCCIOLI, *Atoms, Metaphors* (ct. note 6, ch. 1), p. 52.

systems. For this reason it was legitimate to adopt hypotheses which were not compatible with classical theory. Nevertheless, the determination of the characteristics of electron orbits was made possible only by the formal tools of classical mechanics.

It might seem that Bohr was obliged to follow the procedure he did as both the insufficiency of the classical mechanics and the introduction of a new hypothesis were universally admitted as well as, at the same time, the only formal tools at his disposal were those of classical mechanics itself.

But the explanation cannot be so banal if Albert Einstein wondered about it as Georg de Hevesy reported in a letter to Bohr dated September 23, 1913:

> I have no time yust [sic] now to write you in detail about my experiences here at Vienna Congress but I have to writte [sic] you some very favourable news. I spoke this afternoon Einstein, I asked him first about his formula and he agreed that is hopeless to find differences by diffusion in water, than [sic] I asked him about his view of your theorie [sic]. He told me, it is a very interesting one, important one if it is right and so on and he had very similar ideas many years ago but had no pluck to develop it. I told him that is established now with certenety [sic] that the Pickering-Fowler spectrum belongs to He. When he heard this he was extremely astonished and told me: "Than [sic] the frequency of the light does not depand [sic] at all on the frequency of the electron" – (I understood him so??) And this is an *enormous achievement*. The theory of Bohr must be then wright [sic]. I can hardly tell you how pleased I have been and indeed hardly anything else could make me such a pleasure than this spontaneous judgement of Einstein[5].

As we will see later, Bohr succeeded in deriving from the traditional procedure of energy quantization the physical condition enabling him to get round the obstacles of classical electrodynamics and to determine in quantitatively rigorous fashion the permitted energy levels of any atomic element. At the same time, we cannot pass over

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[5] ROSENFELD, HOYER (eds.), *NBCW*, 2 (cit. note 34, ch. 2), p. 532.

the fact that there remained the problem of the internal consistency of a theory employing classical means in a phenomenal context at variance with the assumptions of the classical theories. The fact did not escape Einstein and this explains the reasons of his surprise.

I intend to analyse the so called two versions of Bohr's theory in order to show the reasons that conducted him to carry on his research program despite the striking contradictions he had to face, which probably would discourage any other scientist. Bohr emphasized the continuity between quantum and classical mechanics, as it was a personal conviction. It is my aim to demonstrate that such emphasis on continuity is twofold. On the one hand, together with the concept of unity, it played a heuristic role for theory construction in its double role of protective mechanism and guiding principle. On the other hand it was an essential condition of the proper understanding of the quantum theory. Which is to say: the key to a harmonious incorporation of the quantum postulate into classical mechanics, to which the next chapter will be devoted.

9.2 A Heuristic for the Construction of the First Atomic Theory

As Bohr himself admitted, "the inadequacy of the classical electrodynamics" is evident if we try to account for "the properties of atoms from an atom-model as Rutherford's". Considering "a simple system of a positively charged nucleus of very small dimensions and an electron describing closed orbits around it", the following hypotheses are introduced[6]:

1. The mass of the electron is negligibly small in comparison with that of the nucleus.

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[6] BOHR, "On the Constitution" (cit. note 34, ch. 2), p. 3.

2. The velocity of the electron is far below the speed of light, which makes it possible to rule out possible relativistic effects.
3. The electron orbiting around the nucleus radiates no energy.

In this particular case, the electron describes elliptical orbits and, denoting the charge of the electron and of the nucleus by *-e* and *E* respectively and the mass of the electron by *m*, it is possible to get the value of the frequency of revolution:

$$\omega = \frac{\sqrt{2}}{\pi} \frac{W_n^{3/2}}{eEm^{1/2}}, \tag{9.1}$$

together with the length of the major axis *a* of the orbit

$$2a = \frac{eE}{W_n}. \tag{9.2}$$

Both the equations are functions of W_n, which is the energy necessary to remove the electron from the orbit under consideration to an infinitely great distance from the nucleus.

The laws of electrodynamics are proved to be inadequate as the problem of the instability rises again if we eliminate the third hypothesis:

«Let us now, however, take the effect of the energy radiation into account, calculated in the ordinary way from the acceleration of the electron. In this case the electron will no longer describe stationary orbits. W will continuously increase, and the electron will approach the nucleus describing orbits of smaller and smaller dimensions and with greater and greater frequency; the electron on the average gaining in kinetic energy at the same time as the whole system loses energy»[7].

[7] *Ibidem.*

In the next step Bohr puts it that, according to Planck's theory of radiation, "the energy radiation from an atomic system does not take place in the continuous way assumed in the ordinary electrodynamics"[8], on the contrary it takes place in distinctly separated emissions. The total amount of energy radiated out from an atomic radiator is equal to $nh\nu$, where n is an entire number and h is a universal constant.

Thus Bohr assumes that the electron, at the beginning of the interaction with the nucleus, is at great distance from it, and he also postulates that the electron, after the interaction has taken place, has settled down in a stationary orbit around the nucleus.

What are the values of the energy of the stationary states?

In order to obtain such values, Bohr has to assume that the process of emission of the radiation is always accompanied by the ionization of the atom. As the case under examination regards the hydrogen, the electron is found after the process of interaction in a bound state n and describes an orbit that is assumed as circular, with $\omega = \omega_n$ and with fixed energy W_n.

In this case, the electron loses a quantity of energy equal to the energy of ionization, which, on the basis of the characteristic of the atomic spectra, is homogeneous.

As Bohr kept the distances from Nicholson's hypothesis of regarding the electrons disturbed during the process of recombination as responsible for the radiation emitted, he had to introduce a further assumption. The last hypothesis resorted to by Bohr – referred to henceforth as the "1/2 hypothesis"[9] – is the first conceptual break in the framework of classical physics because it is the definite renounce to the existence of an immediate relation between optical and mechanical frequencies.

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[8] *Ibid.*, p. 4.

[9] PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), p. 54.

The "1/2 hypothesis" states that the homogeneous radiation emitted is "equal to half the frequency of revolution of the electron in its final orbit"[10]. From Planck's theory we may expect that the amount of energy emitted by the process considered is equal to $nh\nu$, and according to the last assumption $\nu = (1/2)\omega_n$, consequently we obtain:

$$W_n = nh\frac{\omega_n}{2}, \tag{9.3}$$

which is the condition sought for the energy of the stationary orbits. The equations (9.1), (9.2) and (9.3) make it possible to get:

$$W_n = \frac{2\pi^2 me^2E^2}{n^2h^2}, \tag{9.4a}$$

$$\omega_n = \frac{4\pi^2 me^2E^2}{n^3h^3}, \tag{9.4b}$$

$$2a = \frac{n^2h^2}{2\pi^2 meE}. \tag{9.4c}$$

By assigning different integers to n (1, 2, 3…) we obtain a series of values for W_n, ω_n and $2a$ corresponding to a series of configuration of the system, of states in which there is no radiation.

Furthermore, Bohr puts in the above expressions $n = 1$ and $E = e$, and assigns the known values to e, m and h for confirming the compatibility of the theoretical predictions with experimental values. Hence he gets:

$$\begin{aligned} 2a &= 1.1X10^{-8}cm, \\ \omega &= 6.2X10^{15}s^{-1}, \\ W &= 13eV. \end{aligned} \tag{9.5}$$

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[10] BOHR, "On the Constitution" (cit. note 34, ch. 2), p. 4.

After the introduction of the so called "1/2 hypothesis" Bohr devoted some comments to Nicholson's theory, who in an article published at the end of 1912 had shown the possibility to account for "lines on hitherto unknown origin in the spectra of the stellar nebulae and that of the solar corona, by assuming the presence in these bodies of certain hypothetical elements of exactly indicated constitution"[11].

The atoms of these elements are supposed to consist of a ring of a few electrons surrounding a positive nucleus of small dimensions. Nicholson showed that the ratios between the wave-length of different sets of lines of the coronal spectrum can be accounted for by assuming that the ratio between the energy of the system and the frequency of rotation of the ring is equal to an entire multiple of Planck's constant. Despite the excellent agreement between the calculated and observed values, serious problems might be raised against Nicholson's theory. As Bohr pointed out, in Nicholson's theory the frequency of lines in a line spectrum was identified with the frequency of vibration of a mechanical system in a distinctly indicated state of equilibrium. According to Planck's theory, we might expect that the radiation is sent out in quanta, but systems like Nicholson's, in which frequency is a function of the energy, cannot emit a finite amount of homogeneous radiation.

Following the digression on Nicholson's theory, Bohr motivated his "ideas on which the written formulae rest" with the introduction of the two momentous postulates[12], which

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[11] *Ibidem*, p. 6.

[12] In the 1913 article, Bohr introduced the two postulates upon which he was to found his quantum theory of atomic structure and the process of radiation as "principal assumptions". He explicitly recognized them as postulates from 1921 onwards (Bohr, "Zur Frage der Polarisation der Strahlung in der Quantentheorie", *Zeitschrift für Physik*, 1921, 6: 1-9). The content of these postulates underwent continual modifications and conceptual improvements as a result of development of his theory, and especially on the basis of the principle of correspondence.

synthesizes and restates what he had already dealt with in the previous passages of the article.

The assumptions used are:

1. That the dynamical equilibrium of the systems in the stationary states can be discussed by means of the ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis.
2. That the latter process is followed by the emission of a homogeneous radiation, for which the relation between the frequency and the amount of energy emitted is the one given by Planck's theory.

Since the hypotheses are concerned with the possible extension of Planck's ideas, Bohr has to recognize that to each state corresponds the emission of a different number of quanta and, at the same time, there is an unusual relation between the frequency of the radiation and that of the electron.

The first assumption is the confirmation of the limits of the ordinary mechanics, although it holds "in calculations of certain mean values of the motions of the electrons"[13].

The second assumption is in contrast to the ordinary electrodynamics, but, as Bohr admits, is necessary for accounting for experimental facts.

Bohr was aware of the logical inconsistencies of his arguments, that is why at the end of the first paragraph he tries to get around them by introducing the next section with a too much optimistic statement: "We shall, therefore, postpone the discussion of the special assumptions, and first show how by the help of the above principal assumptions, and of

[13] BOHR, "On the Constitution" (cit. note 34, ch. 2), p. 7.

the expressions [9.4] for the stationary states, we can account for the line-spectrum of hydrogen”[14].

As it will be explained in a few lines, the assumptions required [1/2 hypothesis] for deriving the expressions [9.4] contradict the content of the postulates. For this reason Bohr tries to justify the “1/2 hypothesis” with the following observation:

«If we assume that the radiation emitted is homogeneous, the second assumption concerning the frequency of the radiation suggests itself, since the frequency of revolution of the electron at the beginning of the emission is 0»[15].

It can only have meant that the well-known dependence of the optical frequency on the frequency of the oscillator was to be replaced by a new relation in which the frequency of radiation would prove equal to the average of the frequencies of motion of the electron in the initial ($\omega = 0$) and final ($\omega = \omega_n$) states of the process of recombination[16].

Nevertheless, in the light of the quantum conceptions of the time, such hypothesis could not follow at all from the homogeneous nature of the radiation involved. We deduce that Bohr’s choice can be accounted for an *ad hoc* solution regarding the determination both of the atom’s energy levels and of the frequencies of the radiation emitted. As a result, this stratagem enabled Bohr to get a quantitative agreement between mathematical formulas and experimental results.

In this sense, he corrected the initial formula by a factor of ½ when he noted that his calculations did not agree with the experimental values[17].

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[14] *Ibidem*, p. 8.

[15] *Ibid.*, p. 5.

[16] PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), p. 56.

[17] The critical literature has been variously occupied with this problem and formulated a number of different hypothesis: cf., for example, T. Hirosige and S. Nisio, “Formation of Bohr’s Theory of Atomic

Furthermore, it is also possible to raise a more serious objection: even though the "1/2 hypothesis" was self-evident – as Bohr wants to make us believe: "the second assumption concerning the frequency of the radiation suggests itself" – it would be necessary to recognize its inconsistency with the above two postulates. In fact, in these postulates Bohr speaks of generic transitions between stationary states associated with the emission of homogeneous radiation. Conversely, the "1/2 hypothesis" would continue to hold if and only if the averaging procedure remained valid even when the initial frequency is not zero. This condition would be in contrast to any evidence and even to the reasons presented by Bohr: "since the frequency of revolution at the beginning of the emission is zero". In other words, the extension of the condition [9.3] i.e. "1/2 hypothesis" to the determination of the stationary states is possible only if it is admitted that the energy radiation appearing in it is exactly equal to the mechanical energy of a state. But this can be true only if the energy required to remove the electron from that state to an infinitely great distance from the nucleus is found in the form of a field of radiation.

When one speaks of emission via transitions between any stationary states, it does not mean a simple generalization of that condition but rather a conceptual leap, which entails the departure from the physical and theoretical conditions required by the procedure of quantization with which Bohr intended to take up Planck's theory.

Constitution", *Japanese Studies in the History of Science*, 1964, 3: 6-28, which see this procedure as having been suggested by the operation of averaging the energies of the oscillators introduced by Planck in the last formulations of his theory. This interpretation is criticized by Heilbron and Kuhn, "The Genesis of the Bohr Atom" (cit. note 43, ch. 2) who take up Rosenfeld's view and maintain that the idea derives in some way from the fact that in his memorandum Bohr had already obtained a value very close to this; which does not prevent it from looking like "an *ad hoc* rationalization, designed to preserve the parallelism between Bohr's radiator and Planck's" (ibid. 271-272).

If we want to obtain Balmer's formula for the hydrogen spectrum from [9.4a], we have to calculate the difference between the energies of the two states (n_1 and n_2) and divide it by Planck's constant:

$$\nu_n = \frac{W_{n1} - W_{n2}}{h} = \frac{2\pi^2 me^4}{h^2}\left(\frac{1}{n_1^2} - \frac{1}{n_2^2}\right); \tag{9.6}$$

if we put $n_1 = 2$, we get the series in the ultra-red observed by Paschen and previously suspected by Ritz, which was derived empirically by Balmer:

$$\nu = R\left(\frac{1}{4} - \frac{1}{n_2}\right). \tag{9.7}$$

As Bohr observes, the values of the constants of proportionality appearing in the two expressions are equal, within the range of experimental error.

This is a very crucial passage of Bohr's article and some considerations are hence now required:

a. The "1/2 hypothesis" is important for the economy of Bohr's theory to the extent that its generalization entails the equivalence between the energy of radiation and the mechanical energy of the state.

b. Bohr introduces it for getting a quantitative agreement between the energy values of the stationary states and the experimental results, and consequently the forced analogy with Planck's oscillator.

c. The analogy with Planck's oscillator entails the establishment of a quantitatively rigid relation between the energy of the radiation and the frequency of oscillation of an electron.

d. Therefore, the maintenance of the ½ factor is the condition for getting a direct grafting of Planck's ideas onto Rutherford's atomic model, which is to say the road map to establish continuity between quantum theory and a classical-mechanical model as Rutherford's.

e. As I see it, the establishment of such continuity represents the heuristic of the whole Bohr's program, viz. the precondition for what we have termed above the potential for development possessed by classical conceptions and conceptual implications of quantum physics.

As Heilbron and Kuhn have shown, the derivation of Balmer's formula from [9.4a] contains a conceptual trap: "To reach the Balmer's formula Bohr will ultimately change this interpretation, saying instead that equation [9.3] represents the emission of a single quantum with frequency $\tau\omega_\tau/2$"[18]. Bohr's interpretation says that the energy released in radiation by an atomic system of frequency ω is $nh\omega$, and that this emission of energy is the result of distinct and separate elementary processes. Now, by maintaining a strict analogy with such an hypothesis, the energy of each stationary states of the atom should be regarded as composed by indivisible and separate n quanta each possessing $h\omega_n/2$ energy. Moreover, it should be claimed that in each process of radiation this energy is emitted through n distinct and separate successive processes, each of which brings one quantum of energy into play. For this reason, as Bohr hypothesis states, to each stationary state of the atom is associated a different number of quanta, which therefore represents the essential condition for a direct application of Planck's theory to the case of the states of the atom.

[18] HEILBRON, KUHN, "The Genesis of the Bohr Atom" (cit. note 43, ch. 2), p. 270.

In order to derive Balmer's formula it must be supposed that:

$$h\nu = W_{n1} - W_{n2}. \tag{9.8}$$

Where ν is the frequency of the radiation emitted when the electron passes from n_1 to orbit n_2, respectively of energies W_{n1} and W_{n2}, with $W_{n1} > W_{n2}$.

According to experience and Bohr's second postulate:

1. The line observed at frequency $\nu(n_1, n_2)$ corresponds to a homogeneous radiation and it can only be produced by the emission of a single quantum $h\nu$.

2. Therefore, hypothesis [9.8] indicates that, according to the mechanism underlying the atom-radiation interaction, independently of the quantity of energy involved in the process, the energy itself is always released in the form of a single quantum.

3. Ergo this clearly contradicts Planck's hypothesis and therefore also Bohr's.

To summarize, it was after Bohr got acquainted with Balmer's formula that the rigid equivalence between the energy of radiation and the energy of oscillation of an electron proved to be fruitful despite the evident logical and conceptual shortcomings.

As a matter of fact Bohr found:

1. A theoretical value comparable with Rydberg's constant.

2. The explanation of the differences between the spectra of an element observed in laboratory conditions and those observed in the celestial bodies.

3. The attribution of some lines of the spectrum of the star ζ-Puppis to ionized atoms of helium.

Nevertheless, since the empirical confirmations represented very important results for the theory, they rather emphasized the logical contradictions inherent in the analogy with Planck's oscillator.

I would not regard such logical inconsistencies as a trivial matter because the change of Bohr's interpretation concerning the equation [9.3] for getting the Balmer's formula should entail radical modifications of his personal convictions on the relations between quantum and classical physics. On the contrary, as we will see in a few lines, Bohr maintained the analogy between Planck's theory and Rutherford's, although on a formal level. Indeed he assumed that if there exists no evident and immediate relation between optical and mechanical frequencies then the objections to which the previous procedure is open can be avoided by abandoning the physical analogy with Planck's oscillator and adopting a formal analogy with it.

What did lead Bohr to persist in his original conceptual assumptions despite he had to modify the fundamental interpretation on the emission of radiation on the basis of a conceptual trap? I think that the only possible answer is to regard the procedure adopted by Bohr as a heuristic that would have produced results in the subsequent development of the theory.

The next step consists in introducing the second version of the quantum theory of the atom contained in the trilogy and will show once again the heuristic power of Bohr's conceptions.

Bohr opens the third paragraph of the first part of the trilogy with an unequivocal judgement on the untenable nature of some of the hypotheses introduced in the simulated derivation of Balmer's formula.

«We shall now return to the discussion (see p. 7) of the special assumptions used in deducing the expressions [9.4] on p. 5 for the stationary states of a system consisting of an electron rotating a nucleus. For one, we have assumed that the different stationary states correspond to an emission of a different number of energy-quanta. Considering systems in which the frequency is a function of the energy, this assumption, however, may be regarded as improbable; for as soon as one quantum is sent out the frequency is altered. We shall now see that we can leave the assumption used and still retain the equation [9.3] on p. 5 and thereby the formal analogy with Planck's theory»[19].

By retaining the analogy on a purely formal level, Bohr intends to establish a simple proportional relation between his model and Planck's resonator:

$$W_n = f(n) h\omega_n, \tag{9.9}$$

where $f(n)$ is a certain function of the whole variable n to be determined (in the first version, $f(n)$ assumed the value ½); relations analogous to [9.4] are thus obtained:

$$W_n = \frac{\pi^2 me^4}{2h^2 f^2(n)}, \quad \omega_n = \frac{\pi^2 me^4}{2h^3 f^3(n)}. \tag{9.10}$$

By utilizing the hypothesis implicit in [9.8], simple substitution gives:

$$\nu = \frac{\pi^2 me^4}{2h^3}\left(\frac{1}{f^2(n_2)} - \frac{1}{f^2(n_1)}\right). \tag{9.11}$$

Although it implies a drastic reduction of the theory's potential, Bohr describes the next step as follows: "We see that in order to get an expression of the same form as the

[19] BOHR, "On the Constitution" (cit. note 34, ch. 2), p. 12.

Balmer's series we must put f (n) = cn"[20], with c the constant to be determined. Evidently, since [9.11] and

$$\nu = R\left(\frac{1}{n_2^2} - \frac{1}{n_1^2}\right) \tag{9.12}$$

are formally analogous, the unknown f must be a linear function of the discrete variable n. However, in this way the theory abandons all claims to deduce Balmer's formula and to give it a physical interpretation in terms of the mechanism of radiation. On the contrary, the formula is assumed as true since it makes possible to correlate with great precision the numerical data relative to the frequencies of the lines and therefore, once the impossibility of obtaining the conditions of state of the atomic system through a Planck-type quantization procedure has been established, is the only tool remaining to determine properties of formal type[21].

Now the problem consists in the determination of the value of the constant c, for which Bohr takes in consideration a transition of the electron between two contiguous stationary states, characterized respectively by the whole numbers N and N – 1. From [9.11] it follows that this transition involves the emission of radiation of frequency

$$\nu = \frac{\pi^2 m e^4}{2c^2 h^3} \frac{2N-1}{N_2(N-1)}. \tag{9.13}$$

For these two states it is also possible to determine the values of the electron's frequencies of revolution before and after the emission of radiation:

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[20] *Ibidem*, p. 13.

[21] PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), pp. 60-61.

$$\omega_N = \frac{\pi^2 me^4}{2c^3h^3N^3},\ \ \omega_{N-1} = \frac{\pi^2 me^4}{2c^3h^3(N-1)}, \tag{9.14}$$

whose ratio is given by:

$$\frac{\omega_N}{\omega_{N-1}} = \frac{N^3}{(N-1)^3}. \tag{9.15}$$

If N is great – i.e. when the states considered are characterized by low frequencies – the ratio between the frequency before and after the emission will be very near equal to 1; and, Bohr observes, according to the ordinary electrodynamics we should therefore expect that the ratio between the frequency of radiation and the frequency of revolution also is very nearly equal to 1. This condition will only be satisfied if c = ½. From [9.9] it thus follows that the quantum condition for the permitted levels of the hydrogen atom is $W_n = nh(\omega_n/2)$, which is exactly the same as the [9.3] already obtained by Bohr with "1/2 hypothesis" and the rigid application of Planck's hypothesis to Rutherford's model. Bohr developed a procedure valid for the region of high quantum numbers and made possible solely by acknowledgement that, in the presence of transitions between states characterized by a high value of ν, the correspondence between optical and mechanical frequencies, not found in other areas of the spectrum, is re-established. As we have suggested earlier, the claim is, in itself, anything but obvious, if it is true that a formal analogy with Planck's theory led Bohr to re-propose the possibility of the orbiting electron behaving like a Planck's oscillator after he had previously ruled it out in the physical analogy[22].

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[22] Petruccioli noted that Bohr extended to the considered case the well-known theoretical result obtained in the study of heat radiation as regards the agreement of classical electrodynamics with experimental

This fact is crucial because it is an example of the heuristic power either of a personal conviction or of a general judgment on the state of physics, not backed by empirical results but rather in contrast to the fundamental premises of the theory. It follows that the heuristic is able to shape the development of the theory itself.

It is worth remarking that the argument of the limiting region gave no grounds for assuming that the typical discontinuous pattern underlying the mechanism of atomic radiation would disappear in the region of large *n*. On the contrary, Bohr confirmed the quantum character of the process, but he added that the classical predictions are identical to quantum-theoretical predictions from the numerical point of view when the electron, in two continuous stationary orbits, is subjected to motions differing very little one from the other. What we have to stress again is the analogy with a classical theory – in this case of heat radiation – for asserting that optical frequencies tend to assume the same value as mechanical frequencies.

A few years later (1918) Bohr was to formalize and generalize considerations of this type in the principle of correspondence. Many authors, such as Heilbron, Kuhn and Rosenfeld tended to see in this procedure the anticipation or embryo of the principle of correspondence.

As I see it, this interpretation is plausible if we regard such a procedure as guided by a heuristic, which hence underlies the correspondence principle itself.

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data in the low-frequency region of the black-body spectrum. Here Planck's formula became equivalent to the classical Rayleigh-Jeans law and the quantized oscillator returned to rigorously classical behaviour. Nevertheless, Petruccioli recognized that the analogy with the Rayleigh-Jeans law could not be stretched completely to Bohr's case because the law for the density of energy of temperature radiation, as a function of wavelength, agrees with experimental data only in the region of very low frequencies, and that it leads to error if used for high frequencies, where the so-called ultraviolet catastrophe takes place.

9.3 Bohr's Self-Criticism as Regards the Analogy with Planck's Resonator

Bohr had the opportunity to draw up the balance sheet of a year of work in a speech delivered before the Danish Physical Society on December 20, 1913 in Copenhagen. In that occasion he gave a glimpse of the state of contemporary physics, by regarding this as a phase of transition, where new discoveries seemed to cast doubts upon consolidated scientific ideas. His judgments were very prudent. Some of them in particular help us to a better understanding of the main reasons for his change of attitude with regard to the Planckian approach and the reasons, methodological and otherwise, that led him to present on that occasion a new version of the quantum theory of the atom.

«The discovery of these beautiful and simple laws concerning the line spectra of the elements has naturally resulted in many attempts at a theoretical explanation. […] Not one of the theories so far proposed appears to offer a satisfactory or even a plausible way of explaining the laws of the line spectra. Considering our deficient knowledge of the laws which determine the process inside atoms it is scarcely possible to give an explanation of the kind attempted in these theories. The inadequacy of our ordinary theoretical conceptions has become especially apparent from the important results, which have been obtained [with the theory] of temperature radiation. You will therefore understand that I shall not attempt to propose an explanation of the spectral laws; on the contrary I shall try to indicate a way in which it appears possible to bring the spectral laws into close connection with other properties of the elements, which appear to be equally inexplicable on the basis of the present state of the science»[23].

On page 195 of the present chapter I have summarized the three objectives that Bohr intended to achieve within his program. As examination of the first two versions of the theory has shown, Bohr soon found himself obliged to scale down and reformulate the

[23] BOHR, "On the Spectrum of Hydrogen" (cit. note 36, ch. 2), pp. 285-86.

objectives of his program. Indeed, by the end of 1913 the derivation of the Balmer's formula was definitely jettisoned.

This choice was stated openly just in the occasion of his speech addressed to the Danish Society of Physics. The reasons for this choice have a structural character in the framework of the new theory. As Bohr explicitly states, they regard the inadequacy of the ordinary theoretical conceptions with regard to a new phenomenal situation. Shortly afterwards, having run over the stages of the theoretical and experimental research leading up to black-body theory, Bohr takes the opportunity to clarify what positive contribution had come from the physics of Planck and Einstein.

«We are therefore compelled to assume, that the classical electrodynamics does not agree with reality, or expressed more carefully, that it can not be employed in calculating the absorption and emission of radiation by atoms. […] Fortunately, the law of temperature radiation has also successfully indicated the direction in which the necessary changes in the electrodynamics are to be sought»[24].

However, after acknowledging Planck's merits in having pointed to a fruitful idea of research, Bohr's judgments becomes more critical:

«In formal respects Planck's theory leaves much to be desired; in certain calculations the ordinary electrodynamics is used, while in others assumptions distinctly at variance with it are introduced without any attempt being made to show that it is possible to give a consistent explanation of the procedure used»[25].

In the span of a few months Bohr had therefore lost his initial enthusiasm for a theoretical conception, which he had seen, in the introduction to the trilogy, as a way of solving all the difficulties of Rutherford's model. Bohr became fully aware of the

[24] *Ibidem*, p. 288.
[25] *Ibid*.

logical and epistemological weakness that seemed to mark the previous quantum theories, which he viewed as incapable of encompassing all the consequences of the change in conceptual framework required by the new discoveries. Atomic physics was regarded as part of the theoretical mainstream stemming from Planck's work since the acknowledged "fact that we can not immediately apply Planck's theory to our problem is not as serious as it might seem to be"[26].

As Bohr goes on to observe:

«In assuming Planck's theory we have manifestly acknowledged the inadequacy of the ordinary electrodynamics and definitely parted with the coherent group of ideas on which the latter theory is based. In fact in taking such a step we can not expect that all cases of disagreement between the theoretical conceptions hitherto employed and experiment will be removed by the use of Planck's assumption regarding the quantum of the energy momentarily present in an oscillating system»[27].

Nevertheless, "the discovery of energy quanta must be considered as one of the most important results arrived at in physics"[28], in fact the demonstration of the constant *h* makes it possible "at least approximately to account for a great number of phenomena about which nothing could be said previously"[29] implies much more than the validity of the quantitative assumption of a discontinuous transformation of energy.

It must, that is, have a deeper theoretical significance, which cannot as yet be deciphered. For this reason:

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[26] *Ibid.*, p. 292.

[27] *Ibid.*

[28] *Ibid.*, p. 289.

[29] *Ibid.*

«We stand here almost entirely on virgin ground, and upon introducing new assumptions we need only take care not to get into contradiction with experiment. Time, he concluded, will have to show to what extent this can be avoided; but the safest way is, of course to make as few assumptions as possible»[30].

As a matter of fact, by the end of 1913 Bohr paid attention to the consistency between theoretical assertions and experimental data. He also declared his opposition to the uncontrolled proliferation of hypotheses that can be regarded as a criterion guiding the definitive reformulation of the theory.

On the one hand we have the renunciation of any hypothesis as to the behaviour of the systems responsible for emitting radiation, also in view of the fact that "we know little or nothing about these systems. No one has ever seen a Planck's resonator, nor indeed measured its frequency of oscillation of the radiation which is emitted"[31]. On the other, Bohr showed that it is possible to obtain the laws of temperature radiation without making any assumptions about the systems, which emit the radiation. The amount of energy emitted each time shall be equal to $h\nu$, where ν is the observable frequency of radiation.

The theory can be based uniquely on the two following postulates:

1. The first states that the electron is found in a stable state, i.e. without radiating, on an orbit characterized by a certain quantum number both before and after each process of radiation.

2. The second connects each emission and absorption of radiation with a complete transition of the electron from one stationary state to another.

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[30] *Ibid.*, p. 292.

[31] *Ibid.*, p. 293.

If the energies of these states are respectively E_1 and E_2, the following relation gives the frequency of the radiation ν emitted or absorbed:

$$h\nu = E_1 - E_2. \tag{9.16}$$

Together with recognition of the validity of the empirical laws of the spectral lines and Bohr's considerations regarding the limiting region, these postulates are sufficient for the foundation of the quantum theory of the atom. Balmer's formula for the hydrogen spectrum may be written:

$$\frac{1}{\lambda} = \frac{R}{n_1^2} - \frac{R}{n_2^2}, \tag{9.17}$$

where R is Rydberg's constant. Since, as is known, frequency is given by $\nu = c/\lambda$, where c is the velocity of light,

$$\nu = \frac{cR}{n_1^2} - \frac{cR}{n_2^2}. \tag{9.18}$$

Comparing this with (9.16), the energy W of each stationary state is given by:

$$W = \frac{Rhc}{n^2}, \tag{9.19}$$

which, in turn, substituted in (9.1) gives:

$$\omega_n^2 = \frac{2}{\pi^2}\frac{R^3h^3c^3}{e^4mn^6}. \tag{9.20}$$

Bohr goes on to show that one may expect a connection with the ordinary electrodynamics; namely, that it is possible to calculate the emission of slow electromagnetic oscillations on the basis of the classical electrodynamics. Thus he shows that this assumption is supported by the result of Lorentz's calculations. From the formula for ω it is seen that the ratio (ω_n/ω_{n+1}) tends towards 1 as n increases. According to what has been said above, the frequency of the radiation corresponding to the transition between two stationary states n and n+1 is given by:

$$\nu = Rc\left[\frac{1}{n^2} - \frac{1}{(n+1)^2}\right] \tag{9.21}$$

If n is very large this expression is approximately equal to:

$$\nu = 2\,Rc/n^3. \tag{9.22}$$

In order to get a connection with classical electrodynamics Bohr posits this frequency equal to the mechanical frequency, i.e.

$$\omega_n = 2\,Rc/n^3 \tag{9.23}$$

which, substituted in (9.20), supplies the expression of Rydberg's constant:

$$R = \frac{2\pi^2 c^4 m}{ch^3}. \tag{9.24}$$

It is worth noticing that such considerations remain confined to the limited region and no procedure of generalization is attempted:

«We can not expect to obtain a corresponding explanation of the frequency values of the other stationary states. Certain simple formal relations apply, however, to all the stationary states. By introducing the expression, which has been found for R, we get for nth state $W_n = 1/2\ nh\omega_n$. This equation is entirely analogous to Planck's assumptions concerning the energy of a resonator»[32].

Furthermore, Bohr seems to express a sort of self-criticism with regard to the hypothesized possibility of establishing some analogy between the Planckian expression of the energy of the resonator and the energy of the state of the atom. It becomes more explicit when he asserts:

«This analogy suggests another manner of presenting the theory, and it was just in this way that I was originally led into these considerations. When we consider how differently the equation is employed here and in Planck's theory it appears to me misleading to use this analogy as a foundation, and in the account I have given I have tried to free myself as much as possible from it»[33].

Was the self-criticism for the use of the analogy of Planck's resonator a repudiation of the conviction as regards the possibility of the incorporation of the quantum of action into classical mechanics?

This question rises from the fact that within Planck's paradigm Bohr was unable to find such conceptual tools as would legitimize in physical terms the mechanism seen as underlying the atom's radioactive behaviour. Not even in a weak version – i.e. in the context of a formal analogy – can Planck's hypothesis be used to derive the quantum condition making it possible to select the stationary states of the atom, i.e. the physically permitted electronic orbits. As Petruccioli noted, Bohr arrived at this drastic conclusion after his two attempts to develop an analogy between harmonic oscillator and electron rotating on an atomic orbit; attempts which had forced him to burden the theory increasingly with hypotheses that he himself recognized as largely arbitrary.

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[32] *Ibid*., p. 295.

[33] *Ibid*., p. 296.

As I see it, the answer to the above question is in the negative, because Bohr was only recognizing the limits of regarding this analogy as a foundation of the theory, as if it was the only way to obtain the harmonization of the quantum of action within a classical-mechanical model as Rutherford's.

Consequently, it was not a disclaimer of the heuristic use he did of his personal judgment with regard to the continuity between quantum and ordinary physics. Indeed, his purpose was still to demonstrate the validity of such a conviction by searching a further connection with classical mechanics.

The last statement is upheld by the use he did again of the considerations on the correspondence between optical and mechanical frequency, although in the limiting region without proceeding to any generalization. As it has been already shown in the previous passage, in which Bohr posited the frequency of the orbiting electron [9.22] equal to the mechanical frequency [9.23] in order to get the Rydberg's constant.

I think that such interpretation is confirmed by the quotation on ft. 32 in which Bohr affirms that by introducing the expression, which has been found for R, we get for the nth state again the equation [9.3] that is analogous to Planck's assumption concerning the energy of a resonator.

Nevertheless, as these considerations were confined to the limited region and no procedure of generalization were attempted; at the end of 1913 the only proposal Bohr was able to make was a semi-empirical procedure for the determination of the physically possible states of the atom. All the information we can derive is that it represents a practical rule of selection for the atom's energy levels. Moreover, it was capable of providing no other information, especially with regard to:

1. How energy is emitted or absorbed by the atom.

2. The value of the frequency of the radiation that the atom exchanges with the external world.

Unlike Planck, Bohr regarded the quantity of energy involved in each process of radiation as capable of determination not in relation to the state of an electron in its orbit, but on the basis of comparison of the initial and final states of the transition accompanying the radioactive process. It was therefore not the energy of a state – i.e. the energy possessed by an electron in a stable orbit – but rather the variation in energy in the passage of an electron from one state to another that was equal, minus the constant h, to the frequency of radiation. Bohr's theory was hence incapable of saying anything about the nature of that discontinuous process in which the system passes from one stationary state to another. The theory was also incapable of saying what happens at the moment in which we disturb an atom with radiation. The concept of discontinuity was thus introduced into atomic theory. With regard to it and to the general theoretical consequences it entailed, Bohr was to repeat in all his writings subsequent to 1913 that until a precise idea was obtained of the process of radiation it would be quite arbitrary to assert the existence of an incompatibility in principle between quantum and classical physics.

Conversely, he was still convinced of the possibility of ascertaining the validity of their compatibility. This conviction shall characterize the subsequent development of Bohr's quantum theory.

Chapter 10

Rational Generalization Thesis and Correspondence Rule

10.1 At the Origin of the Correspondence Principle

When Bohr was preparing the lecture to address to the Como Conference in commemoration of Alessandro Volta, on September 1927, he wrote some preparatory notes, which may throw some light on the origin of the principle of correspondence. This is a document of three handwritten pages contained in the folder "Como Lecture II (1927)". The text is in Danish and was written by Bohr. It contains also a final addition of two lines probably written by Oskar Klein.

The paper presented by Bohr at the Conference summarizes the phases of the development of his research program, which led to the idea of complementarity.

Complementarity was seen as the only solution Bohr could give in quantum terms to the general problem of scientific knowledge based upon space-time pictures.

In the preparatory notes Bohr presented an atomic theory in which space-time images were utilized, but within certain limits and with great prudence. As a consequence, Bohr recognized the validity of the classical theory of radiation, by reducing the hypothesis of light quanta to a formal device for the interpretation of certain phenomena.

At the same time, he also admitted the legitimacy of the mechanical description of the electron despite it entailed the violation of ordinary electrodynamics.

Nevertheless such descriptions could not explain the cause of the radioactive behaviour of the atom. The reasons were to be found in the hypothesis of quantization that was incompatible with any traditional model of description.

Bohr was able to establish only a formal analogy between the radiation field and the motion of the orbiting electron.

The above relation brought to light the discontinuous nature of the process connecting the two phenomena involved in the formal analogy.

As we have already shown, and the preparatory notes confirm it, from the outset of his atomic theory Bohr tried to connect the statistical laws with the properties of the models adopted: i.e. a way for ascertaining if and how classical conceptions were applicable to quantum phenomena.

This is to say that Bohr was still guided by what we have termed a heuristic for the atomic theory's construction: the fundamental continuity between classical and quantum physics.

This is the text of the translation of the first page of the document:

I 10-7-1926 [1927][1]

All information about atoms expressed in classical concepts.

All classical concepts defined through space-time pictures.

Therefore beginning of quantum [theory?] piecewise use of space-time pictures formally connected by relations containing Planck's constant. and on conservation of energy and momentum.

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[1] Rüdinger and Kalckar claim that the date (10.07.1926) is wrong in that certain references contained in the document show it to date from July 1927. An anastatic copy of the three pages, transcription and English translation are to be found in KALCKAR (ed.), *NBCW*, 6 (cit. note 1, ch. 1), 59-65.

The connection of essentially discontinuous and statistical kind. *The endeavours at connecting the statistical laws with the properties of pictures* thus implied that *they appeared as generalization of the classical theory*, and in particular converge to the demands of this theory in the limit [where] in statistical applications one may disregard the discontinuous element. Led to the recognition of *a far-reaching correspondence between the quantum theory and the classical theory* and to the *program of developing a [consistent?] quantitative description [by] looking for analogous features in the classical theory*. However it proved impossible to express this quantitatively by space-time pictures. [Indeed?], the theory exhibited a duality when one considered on the one hand the superposition principle and on the other hand the conservation of energy and momentum[2].

It was in such a framework of ideas that the correspondence principle was formulated on the basis of the recognition that in the limiting region of high quantum numbers – where the element of discontinuity may be overlooked in statistical applications – classical predictions are in quantitative agreement with experimental data. Bohr assigned to this principle a central function in the construction of his theory. But I want to highlight that the properly said "correspondence principle" was primary a technical tool (i.e. properly inherent to physics) – at least by its formulation of 1918 – that derived from the initial conviction that later became recognition of "a far-reaching correspondence between the quantum theory and the classical theory", as the italics in the above quotation clearly state. In addition, we cannot pass over the fact that the first time Bohr used the correspondence argument, although with non-negligible differences, was in the first article of the trilogy.

[2] ID., *NBCW*, 6 (cit. note 1, ch. 1), p. 61.

As we have seen in the previous chapter, authors such as Rosenfeld, Kuhn and Heilbron regarded considerations concerning the region of high quantum numbers as an anticipation of the principle of correspondence.

This interpretation is backed by the fact that the limiting considerations, upon the correspondence relation is based, are already present in Bohr's earliest work. On the contrary, other authors (Sandro Petruccioli, for instance) affirmed that the consistent formulation of the principle of correspondence rather implies a logical relation not to be found in the procedure followed in 1913[3]. In fact, Petruccioli sustains that Bohr made an intuitive leap from the 1913's procedure to the formulation of the correspondence principle that was translated into two generalizations.

The first consists in asserting that in the limiting region the connection found did not regard solely the values of the frequencies since the spectrum would reflect the nature of the particle motion in full. In short, it was to be expected that the analogy would be respected in general. The second generalization extends the validity of the relation of correspondence also to cases in which there was no longer any numerical identity to be found between optical and mechanical frequencies and where it proved impossible "to obtain a simple quantitative direct connection between the probabilities of the various transitions and the motion"[4]. Here, Bohr was upheld only by his intuition, which helped him to assert that "we are led to consider the possibility of the occurrence of a transition between two given stationary states as conditioned by the appearance in the motion of

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[3] PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), pp. 87-88.

[4] BOHR, "L'application de la théorie des quanta aux problèmes atomiques" (On the Application of the Quantum Theory to Atomic Problems), in *Atomes et électrons, Rapports et discussions du Conseil de Physique tenu à Bruxelles du 1er au 6au Avril 1921* (Paris: Gauthier-Villars, 1923), 228-47. The original English is reproduced in ROSENFELD, RUD NIELSEN (eds.) *Niels Bohr Collected Works* [NBCW], Vol. 3: The Correspondence Principle, 1918-1923 (Amsterdam: North-Holland Publishing Company, 1976), 364-80, p. 376.

the corresponding harmonic vibration"[5]. Bohr, of course, was not able to say in which motion of the system this harmonic should appear to influence the occurrence of a process of transition.

As I see it, it is possible to find a synthesis between the above mentioned two antithetic positions if we recognize that Bohr was inspired by a general conviction with regard to the relation between quantum and ordinary physics. If we accept such interpretation we shall consider the correspondence principle neither as a formal analogy nor so radical a revision of the procedure followed in 1913. According to this interpretation, I think that Petruccioli is right in the sense that he regarded the correspondence principle as a "physical principle" (i.e. the quantitative convergence found between spectrum and motion but more general than 1913's) applicable in a well-defined domain and, at the same time, he stressed the differences between the first formulation concerning the concept of correspondence of 1918, based on the analogy between classical and quantum mechanics, and the later formulation of 1920.

Kuhn, Rosenfeld and Heilbron would be also right in claiming a conceptual contiguity between the 1913 and the later formulation of the correspondence principle if they considered it as a *universal* principle, i.e. non-applicable only in a limited domain. Indeed Bohr himself declared that the examination of atomic problems "has given unrestricted and convincing support"[6] for the *viewpoint* that he summarized under the name of the principle of correspondence.

It follows that both the 1913's embryonic version of correspondence and the subsequent formulations developed by 1918 are to be considered as gradations of the same

[5] *Ibidem.*

[6] *Ibid.*, p. 377.

universal conception of quantum theory as a rational generalization of the classical theory. Conception that is essential for the proper understanding of the whole quantum mechanics. I shall clarify this position in the next paragraphs.

Moreover, we have to remind ourselves that correspondence is also expression of a heuristic or methodological precept guiding the formulation of a new theory. This conclusion confirms that even the 1913's procedure has to be considered as part of the heuristic underpinning the construction of the new theory.

10.2 The Twofold Formulation of the Correspondence Principle

The first work on which Bohr used the concept of correspondence was "On the Quantum Theory of Line-Spectra. Part I", published in 1918:

«[…] in the limit where the motions in successive stationary states comparatively differ very little from each other, will tend to coincide with the frequencies to be expected on the ordinary theory of radiation from the motion of the system in the stationary states»[7].

In this statement Bohr went over and generalized the considerations regarding the limiting region introduced in the trilogy. In the years before Bohr adopted the expression "correspondence principle", he used the considerations as regards the limiting region as a sort of analogy between classical and quantum mechanics. In his later writings Bohr explicitly rejected this view that the correspondence principle can be thought of as an analogy between the two theories.

The first occasion on which Bohr used the words "correspondence principle" was in "On the Series Spectra of Elements" from 1920 (lecture before the German Physical

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[7] ID., "On the Quantum Theory of Line-Spectra" (1918), *Det Kgl. Danske Vid. Selsk. Skrifter. Naturvidenskabelig og Matematisk Afdeling*, Række 8, IV. Copenhagen 1918-1922, 5-36, p. 8. Reprinted in *NBCW*, 3 (cit. note 4), 71-102, pp. 73-74.

Society in Berlin, April 27). The correspondence program was finally to assume definitive shape in the paper that, on Lorentz's invitation, Bohr presented at the third Solvay Conference in April 1921[8]. Bohr did not attend the Brussels meeting in person and entrusted Ehrenfest with the task of illustrating the paper's main points and representing his views during the discussion, which concentrated almost exclusively on the meaning and the applicational consequences of the new principle[9]. Bohr's absence was due to poor health.

The paper dealt with two subjects: the determination of the conditions of state for the selection of permitted energy levels on the basis of the properties of motion possessed by an atomic system in a given stationary state; and the examination of the problem of interaction between radiation and matter from the quantum theoretical viewpoint. It was in the latter context that Bohr introduced the principle of correspondence. The whole discussion was developed within the framework of reference defined by the fundamental postulates, which Bohr reformulated in the following terms:

«An atomic system which emits a spectrum consisting of sharp lines possesses a number of separate distinguished states, the so-called stationary states, in which the system may exist at any rate for a time without emission of radiation, such an emission taking place only by a process of complete transition between two stationary states […]. In the theory, the frequency of radiation emitted during a process of this kind is not directly determined by the motion of the particles within the atom in a way corresponding

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[8] ID., "On the Application" (cit. note 4).
The conference was chaired by Lorentz and partecipants included: C. G. Barkla (Edimburg), W. L. Bragg (Manchester), M. and L. Brillouin (Paris), L. de Broglie (Paris), M. Curie (Paris), P. Ehrenfest (Leyden), W. J. De Haas (Delft), H. Kammerlingh Onnes (Leyden), M. Knudsen (Copenhagen), P. Langevin (Paris), J. Larmor (Cambridge), R. A. Millikan (Chicago), J. Perrin (Paris), O. W. Richardson (London), E. Rutherford (Cambridge), M. Siegban (Lund), E. Van Aubel (Ghent), P. Weiss (Strasburg), and P. Zeeman (Amsterdam). A. A. Michelson (Chicago), then in Europe, was also invited. Besides Bohr, others unable to attend were W. H. Bragg (London), A. Einstein (Berlin) and J. H. Jeans (Dorking). Cf. J. MEHRA, *The Solvay Conferences on Physics* (Dordrecht: Reidel, 1975), ch. VI.

[9] PAUL EHRENFEST, "Le principe de correspondance", in *Atomes et électrons* (cit. note 4), pp. 381-87.

to the ideas of the classical theory of electromagnetism, but is simply related to the total amount of energy, emitted during the transition […]»[10].

At this point he introduced the general relation frequencies:

$$h\nu = E' - E'' \tag{10.1}$$

which he regarded as the formal basis of quantum theory.

Once the existence of an irreparable break with the customary ideas of physics had been recognized, the general objective of the theory became for Bohr the systematic exploration of the possibility of successfully developing a formal analogy with those ideas. The first step in this direction consisted in asking how far the quantum postulates made it possible to describe motion in stationary states with the concepts used classically to describe the behaviour of a system of charged particles. In other words, Bohr intended to ascertain with what degrees of approximation it was still possible "to describe the motion of the particles in the stationary states of an atomic system as that of mass points moving under influence of their mutual repulsion and attraction due to their electric-charges"[11]. That this was, in any case, not permitted in examining the external disturbances of particle motion was a consequence of quantum problem of the stability of the stationary states.

The theoretical conditions for their selection among the possible mechanical motions of the system referred in fact to properties dependent on the periodicity of the orbits and not on the velocities and configurations of the particles. In the case of an atomic system subjected to variable external conditions, the theory was therefore obliged to abandon

[10] BOHR, "On the Application" (cit. note 4), pp. 365-66.

[11] *Ibidem*, p. 367.

the approach of ordinary mechanics, which would entail the study of the effects of the forces acting on the particles at a given instant.

The theory would rather be required to determine how much conditions modified the properties of periodicity of a state and thus to arrive at the orbital motion of particles that would be compatible with them. An example of the behaviour of atoms under the action of variable external conditions was given by the phenomena of light absorption and emission, which provided further confirmation that "the interaction of the atom with the incident electromagnetic waves can by no means be described on the basis of the classical electronic theory"[12].

At the time Bohr saw in the "unknown mechanism" responsible for the process of radiation the principal reason for the existence of an insuperable limit to the descriptive possibilities of the classical concepts. Although the theory provided no explanation on this point, the phenomena examined did make it possible to clarify why a rigorously classical treatment of the process of interaction was forbidden: in such phenomena the external forces undergo significant alterations within periods of time that cannot be compared with the periods characteristic of the motion of atomic particles[13].

We could say that Bohr intended to ascertain whether the mechanical analysis of electron motion would produce significant results even when one gave up the idea of regarding the atom as an isolated system, and try to determine how far a mechanical description of the model was still admissible. Bohr used the following rational tools to achieve his purpose: Ehrenfest's adiabatic principle and Sommerfeld's formal rules for the determination of stationary states. Sommerfeld was able to account for the fine

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[12] *Ibid.*, p. 368.

[13] Cf. PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), pp. 83-84.

structure of hydrogen by incorporating relativistic considerations (the variation of the electron mass in its elliptical orbit) in the generalized quantum conditions.

By doing so, the old quantum theory could be applied not only to simply periodic motion, but to so-called conditionally (or multiply) periodic motion as well. The third tool was the Epstein-Schwarzschild theory, which made it possible to extend the conditions of state to the so-called multi-periodic motions, i.e. to a set of system more complex than those considered at first, such as the hydrogen atom, by for which the equations of motion could still be solved with the method of separation of variables[14].

Furthermore, Bohr found that, within the range of approximation required by a rigorous mechanical treatment of the motion of atomic particles, the theory's interpretative effectiveness was, in any case, somewhat reduced, since most of the phenomena examined implied the existence of physical conditions contrasting with such approximations. However, Bohr defended his mechanical/model-based approach, stressing that there was at the time no other tool capable of providing an unambiguous definition of the energies of the states appearing in the general relation of frequencies:

«At the present state of the theory we do not possess any means of describing in detail the process of direct transition between two stationary states […]»[15].

The shortcoming that Bohr saw in the theory assumed still greater importance when one went on to examine the process of radiation, in which the initial assumptions made it

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[14] Bohr was to develop these arguments rigorously in the article of November 1922, "Über die Anwendung der Quantentheorie auf den Atombau", *Zeitschrift für Physik*, 1923, 13: 117-65. As was pointed out in a note, the article, which dealt with the fundamental postulates of the theory, was to have constituted the first of a series of papers under the same title in which Bohr intended to deal sistematically with the problems connected with the study of atomic structure. The article was translated into English by the American physicist L. F. CURTIS: "On the Application of the Quantum Theory to Atomic Structure", *Proceedings of the Cambridge Philosophical Society* (Supplement) (1924), 1-42.

[15] ID., "On the Application" (cit. note 4), p. 372.

necessary to renounce any attempt to establish a direct connection between particle motion and radiation and to go no further than the hypothesis that the individual components of the spectrum were due to the occurrence of a certain number of independent processes within the atom. This was decidedly meagre information, especially if one believed, as Bohr then maintained, that the full understanding of such processes had to be subordinated to the construction of "a detailed picture of production and propagation of radiation"[16].

After over twenty years of studies and of undeniable success, the problem facing quantum theory was still the same: the absence of a real understanding of the interaction between radiation and matter. And it was with regard to this point that Bohr, called for the first time to take his place in the most prestigious group of physicists of the day, maintained that the strategy to pursue was still the search for a unified picture for the mechanism of emission and absorption and for the propagation of radiation through space. Immediately afterwards however, almost as though to tone down the pessimistic conclusions drawn by his analysis, he stated:

«We shall see, however, how it is possible to trace a connection between the motion of an atomic system and the spectrum which, even if it must be essentially different from that which would follow from the classical electromagnetic theory, still preserves such features that it gives us hope of attaining a picture which includes the interpretation of the experimental evidence regarding atomic processes as well as the phenomena of interference of light waves […]»[17].

This hope was certainly not such as to make him change his drastic judgement as regards the break with classical theory brought about by quantum theoretical

[16] *Ibidem*, p. 374.
[17] *Ibid*.

conceptions. As he put it, the mechanism underlying the new picture would most probably entail a revision of the fundamental concepts of physics themselves. In other words, Bohr's intention was not to save some fragment of theory, even if his words made it clear that the general viewpoint was still that of a field conception of electromagnetic phenomena. He was concerned rather with demonstrating the existence of sufficient evidence to relate quantum processes to the classical model of description.

Once again, in the absence of suitable tools to tackle the problem of the transition mechanism directly, he found in the limiting region the formal condition enabling him to identify "a certain suggestive connection between the transitions and the motion of the system"[18]. In this region thanks to the methods of analytical mechanics, a quantitative convergence was found between spectrum and motion even more general than that discovered in 1913. It could, in fact, be demonstrated rigorously that for multi-periodic system there exists a simple relation between the frequency of a harmonic component of motion in all transitions between states for which the values of the respective principal quantum numbers, *n'* and *n''*, are large with respect to their difference. This made it possible to connect the occurrence of a transition with the properties characteristic of motion even if, as Bohr again pointed out, this did not mean a progressive elimination of the differences between the quantum nature of the radiation process and classical ideas.

Within the idea of a correspondence between processes of transition and components of motion lay a logical relation which Bohr regarded as capable of establishing a dependence between spectrum and motion similar in all respects to that whereby in

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[18] *Ibid.*, p. 375.

classical theory the intensity of the radiation emitted by a particle in the course of a harmonic oscillation depends upon its amplitude.

In the course of a lecture delivered in September 1923 at the British Association Meeting in Liverpool, Bohr reformulated the second postulate of his theory in the light of his new principle and, whereas it had hitherto asserted the impossibility of deriving the frequencies of the radiation emitted from the motion of the particles, it now stated that "a process of transition between two stationary states can be accompanied by the emission of electromagnetic radiation, which will have the same properties as that which would be sent out according to the classical theory from an electrified particle executing a harmonic vibration with constant frequency"[19].

This was an important amendment, considering that, before the American physicist John Slater arrived in Copenhagen, Bohr had already matured, on the basis of the correspondence principle, the idea underlying the theory of virtual oscillator, the final attempt to save the classical model of description.

10.3 The Theory of Virtual Oscillators

The theory of virtual oscillator is as an example of Bohr's peculiar conception of the principle of correspondence. In fact, in the article published on January 1924, "The Quantum Theory of Radiation", signed together with John Slater and Hendrik A. Kramers, Bohr arrived to this conclusion:

«At the present state of science it does not seem possible to avoid the formal character of the quantum theory which is shown by the fact that the interpretation of atomic phenomena does not involve a description of the mechanism of the discontinuous process […]. On the correspondence principle it seems

[19] ID., "The Correspondence Principle", in *Report of the British Association for the Advancement of Science*, Liverpool, 1923, 428-29. Reprinted in *NBCW*, 3 (cit. note 4), 576-77, p. 576.

nevertheless possible to […] arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field […]»[20].

This passage announced a turning point in atomic physics. On the basis of the researches carried on, discontinuity was not in itself considered as incompatible with the theory's descriptive content, and rational solutions were provided that related a whole set of phenomena dependent on the radiation properties of matter to the continuous picture of the radiation field.

I want to point out the divergence between the three scientists due fundamentally to the different weight assigned to the principle of correspondence. If it is true that Bohr and Kramers[21] were interested in the scheme Slater proposed[22], what they saw in it was not only quite contrary to his intentions but something he could not understand very clearly:

«I got started a couple of days after Christmas telling about this theory and that has got them decidedly excited. I think, of course, they don't agree with it all yet. But they do agree with a good deal, and have no particular arguments except their preconceived opinions against the rest of it, and seem prepared to give those up if they have to. So I think there are hopes»[23].

This was not at all the case; within a few weeks a new theory was worked out that left their differences of opinion unaltered, which can be gathered among other things from

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20 BOHR, HENDRIK A. KRAMERS, JOHN C. SLATER, "The Quantum Theory of Radiation", *Philosophical Magazine*, 1924, 47: 785-802, pp. 785-86; Reprinted in STOLZENBURG (ed.), *Niels Bohr Collected Works*, Vol. 5: The Emergence of Quantum Mechanics, 1924-26 (Amsterdam, NL: North Holland, 1984), 101-18, pp. 101-2; the German version of the article, "Über die Quantentheorie der Strahlung", *Zeitschrift für Physik*, 1924, 24: 69-87.

21 The crucial role that the correspondence principle played in the B.K.S. theory is worth mentioning: Landenburg used it in his justification of the idea of virtual oscillators; it made it possible to estimate the values of the transition's probabilities and of the life time of the stationary states. Moreover, it gave Bohr an argument for rejecting the hypothesis of the light quanta.

22 Slater's idea was that a classical mechanism was at work in the atom that could reconcile, in quantitative terms as well, the statistical character of quantum emission processes with the customary treatment of the elecromagnetic field.

23 Letter from Slater to his parents, July 27, 1924, cit. in STOLZENBURG, "Introduction to Part I", in *NBCW*, 5 (cit. note 20), 1-96, p. 7.

the letter Slater sent from the Copenhagen Institute for Theoretical Physics to Nature on January 28. Slater did not intend to publicize his dissent, but, at the same time, he did not accept the critics of his colleagues:

«The idea of the activity of the stationary states presented here suggested itself to me in the course of an attempt to combine the elements of the theories of electrodynamics and of light quanta by setting up a field to guide discrete quanta, which might move, for example, along the direction of Poynting's vector. But when the idea with that interpretation was described to Dr. Kramers, he pointed out that it scarcely suggested the definite coupling between emission and absorption processes which light quanta provided, but rather indicated a much greater independence between transition processes in distant atoms than I had perceived. The subject has been discussed at length with Prof. Bohr and Dr. Kramers, and a joint paper with them will shortly be published in the *Philosophical Magazine*, describing the picture more fully, and suggesting possible applications in the development of the quantum theory of radiation»[24].

The article appeared under the signature of the three authors in spite of Slater's standpoint was scarcely represented. Otherwise it would not be possible to comprehend the reasons, which led Slater to stress once again his original hypothesis to establish "a harmony between the physical pictures of the electro-dynamical theory of light and the theory of light quanta"[25]. Furthermore, why did he feel the need to write in a letter to Van Vleck that the article was written entirely by Bohr and Kramers?

It is worth noticing that once Slater's idea had been reconsidered in the light of the correspondence principle, there was very little left of the classical mechanism with which he had tried to solve the problem of the production and propagation of radiation. Even though Slater pointed out his colleagues' preconceived rejection of the corpuscular picture of radiation, we have to remind that his scheme contained a serious

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[24] SLATER, "Radiation and Atoms", *Nature*, 1924, 113: 307-08.

[25] Letter of Slater to Van Vleck, July 27, 1924, in *NBCW*, 5 (cit. note 20), p. 20.

logical inconsistency. As Petruccioli noted, Kramers' criticism concerned the presence of a theoretically unjustified asymmetry in the scheme: if the amplitudes of the fields associated with a stationary state determine the probabilities for the emission of a quantum and therefore of the transition to a different state, then something very similar should occur in transitions involving the absorption of a quantum. In any case, if the field acts on transitions solely by defining their probabilities, Kramers was right in pointing out to Slater that his proposal necessarily implied an independence of the transition processes in two distant atoms. It is in fact entirely arbitrary to affirm that the second atom must absorb the same quantum that the first atom emits[26].

As far as Bohr's view is concerned, on the one hand he acknowledged Slater's merit in suggesting the fundamental idea; on the other hand, he wanted to point out that it was thanks to the correspondence principle that a connection had been established between the discontinuous processes occurring inside the atom and the continuous character of the radiation field.

To summarize, one of the main reasons of disagreement between Bohr, Kramers and Slater was not whether light quanta existed or not, but rather the incompatibility of Slater's approach and Bohr's conviction that the concept of virtual field would facilitate an important generalization of the correspondence principle.

Petruccioli outlined three main points which brings evidence to such a claim:

1. Kramers' initial criticisms were not at all directed at the picture Slater had suggested for the propagation of radiation (waves drawing the light quanta along), but rather at the mechanism of radiation.

[26] Cf. PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), p. 115.

2. The use of the term "virtual" to indicate the fields emitted by the oscillators associated with stationary states is not indisputably Slater's, since there is no trace of it in his writings prior to his arrival in Copenhagen. He spoke of "virtual fields" in his letter to *Nature*, which however was written when the article was practically complete. In any case, we shall see that in light of Bohr's interpretation based on the correspondence principle, Slater's fields must be considered entirely real.
3. In Slater's scheme the correspondence principle plays a marginal role and is taken up only to determine the amplitudes of the oscillations corresponding to the various emission frequencies. Its function, in any case, is certainly not that of linking the continuity of the field with the discontinuity of the quantum processes. The reference Slater makes to statistical thermodynamics shows rather that he considered the probabilistic nature of the transitions to be a temporary characteristic of the theory due to incomplete knowledge of atomic processes and not an intrinsic element of quantum phenomena[27].

Bohr presented the new quantum theory of radiation as a further development of the correspondence program, with the purpose of establishing a continuity, which was not just hypothetical:

«The present paper may in various respects be considered as a supplement to the first part of a recent treatise by Bohr, dealing with the principles of the quantum theory, in which several of the problems dealt with here are treated more fully»[28].

[27] *Ibidem*, pp. 115-16.

[28] BOHR, KRAMERS, SLATER, "The Quantum Theory" (cit. note 20), p. 786.

The importance of the new theory was due to the formation of "a picture as regards the time-spatial occurrence of the various transition processes on which the observations of the optical phenomena ultimately depend"[29]. It must be recognized that the only progress achieved was the interpretation of observed radiation phenomena "by connecting these phenomena with the stationary states and the transitions between them in a way somewhat different from that hitherto followed"[30].

This was possible because

«[…] The correspondence principle has led to comparing the reaction of an atom on a field of radiation with the reaction on such a field which, according to the classical theory of electrodynamics, should be expected from a set of "virtual" harmonic oscillators with frequencies equal to those for the various possible transitions between stationary states»[31].

Moreover, the same correspondence principle suggests two hypotheses, which define the modes of this comparison:

1. A given atom in a certain stationary state will communicate continually with other atoms through a time-spatial mechanism which is virtually equivalent with the field of radiation which on the classical theory would originate from the virtual harmonic oscillators corresponding to the various possible transitions to other stationary states.

2. The occurrence of transition processes for the given atom itself, as well as for other atoms with which it is in mutual communication, is connected with this mechanism by probability laws which are analogous to those which in Einstein's

[29] *Ibid.*, p. 791.
[30] *Ibid.*, pp. 789-90.
[31] *Ibid.*

> theory hold for the induced transitions between stationary states when illuminated by radiation[32].

This passage is fundamental in the history of atomic physics because what arises from the first hypothesis is a point of divergence for historical interpretation of Bohr's work, and of the evolution of the Copenhagen school of thought.

One has not to consider such a model as an extreme attempt to save a research program, as the hypothesis Bohr wanted to test has nothing to do with the orbit model or with Maxwell waves. In fact, on the basis of the correspondence principle, Bohr established a virtual equivalence between the description of the virtual model and the real physical situation of quantum systems.

The second hypothesis referred explicitly to the probabilistic considerations Einstein had used in 1917 to arrive at a new derivation of Planck's formula of heat radiation starting from the notion of quantum transition. According to Einstein, there were two distinct emission processes, one which took place spontaneously when the system was in an excited state, and another which was induced by the presence of a radiation field of the suitable frequency[33]. Therefore, Bohr appeared to make a simplification of Slater's hypothesis by eliminating every reference to electromagnetic energy fluxes and light quanta, and hence took advantage of the analogy with Einstein's theory to give it a more general formulation: the probability of every transition is determined by fields produced by virtual oscillators. As a consequence, it became evident that, following this interpretation, the "spontaneous emissions" were transitions induced by virtual radiation

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[32] *Ibid.*, pp. 780-91.

[33] ALBERT EINSTEIN, "Zur Quantentheorie der Strahlung", *Physikalische Zeitschrift*, 1917, 18: 121-28.

fields. But the implications of such assumption undermined the rational foundations of the understanding of nature.

In fact, in the virtual oscillator model, the interaction of radiation and matter is described on the basis of the activity of the virtual fields, which establish communication between one atomic system and another. One has to imagine that in the model the quantum stationary states are replaced, on the basis of correspondence, by a system of charges oscillating at the transition frequencies allowed by the state, and that in general every field – generated according to classical laws by oscillators – acts probabilistically in Einsteinian fashion on the transitions between states. It obviously follows that a process of transition will depend both on the virtual fields of its own oscillators which correspond to the initial state of the transition and on all the communicating fields of atoms. Furthermore, the occurrence of one transition does not influence the transitions taking place in the other atoms; in fact, during the entire process the atom does not emit any virtual fields.

As a consequence, we are obliged to abandon "any attempt at a causal connection between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories"[34].

In fact, if an atom *A* can contribute to the occurrence of a transition in an atom *B* through its own virtual fields, it means that the events do not show any causal relation and, above all, all the quantities of energy involved in the transitions of the atoms do not

[34] BOHR, KRAMERS, SLATER, "The Quantum Theory" (cit. note 20), p. 791.

satisfy the principle of the conservation of energy[35]. Finally, the article concluded that the observed validity of the conservation principle cannot be anything but the result of a statistical average over a great number of individual events, for which these principles are not rigorously satisfied. It is beyond doubt that the model provides a space-time description of the interaction of radiation with matter, although it violates the principle of the conservation of energy and the concept of causality. Nevertheless, it was "the only consistent way of describing the interaction between radiation and atoms by a theory involving probability considerations"[36]. However, the model could not be regarded as the result of an attempt to represent coherently the real physical situation of atomic processes. But this was not the goal Bohr intended to achieve by means of the

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[35] The renouncement of energy and momentum conservation implied a revolution in the classical conception of causal (in the sense of deterministic) natural science. The idea of giving up the classical notion of causality as the law of cause and effect for the radiation processes was, in particular, defended by Kramers, and it was (with Bohr's support) incorporated into the theory against Slater's will. Professor Hans Radder showed how this plea for the acausal character of the B.K.S. theory is related to Kramers' more personal views of man and the world. For instance, Kramers wrote: «At the institute we have been extremely busy lately and the romantic side of my soul is covered with dust and cobweb» [Letter from Kramers to Romein, April 29, 1923]. The way in which he then viewed the relation between his scientific work and this "romantic side of his soul" is explicitly and unambiguously expressed by him as follows: «In my field I struggle for the broadest points of view; will I ever be so fortunate as to reach a new mountain top? Perhaps, just perhaps, but one thing is certain; never shall I be able to fathom what precisely were the struggle and the victories in the life of my best friend. His line of thought, his sensing and understanding of the world I shall never come to know. Here the intellect can accomplish nothing, the instinct a little, love alone everything. […] I think you surely will understand what I mean. It is insane to want to grasp the world in the manner of the rationalists. This becomes clear to you as soon as you have once really tried, and succede in grasping just something in that manner. […] That we believe in what we persue makes life possible, anything understood to be true can be understood to be false by Mephistopheles, and God does not think about removing him. […] For, I would not like to pass for an intellectual; I very seriously mean everything that I have written» [Letter from Kramers to Romein, October 28, 1924]. It was noted that we are not dealing with some isolated remarks of Kramers. They can be understood as being connected, on the one hand, with his musical and literary interests and gifts and, on the other hand, with his philosophical view that science does describe the formal aspects, but not the essence of phenomena. It is also worth noticing Kramers' inclination for the romantic culture that he shared with Bohr. This could explain why they ascribed so much importance to the correspondence principle. This could be stand for a point in favour of the claim that the correspondence principle in their view is underpinned by an extra scientific factor, which was not recognized by Slater because of a different background. From HANS RADDER, "Between Bohr's Atomic Theory and Heisenberg's Matrix Mechanics. A Study of the Role of the Dutch Physicist H.A. Kramers", *Janus*, 1982, 69: 223-252, pp. 235-36.

[36] BOHR, KRAMERS, SLATER, "The Quantum Theory" (cit. note 20), pp. 792-93.

application of the principle of correspondence, as what exists between atom and model is a virtual equivalence and not a real correspondence[37].

Professor Petruccioli noted correctly that Slater's position couldn't be more distant. In fact, Slater held that the fields produced by oscillators are real objects that draw energy particles through space. On the contrary, for Bohr, the model is a purely logical tool, a theoretical fiction, which, though constructed within a conceptual framework irreducible to the quantum-theoretical, can nevertheless enable us to explore certain aspects of the reality of atoms. In particular, Bohr appeared to ask himself about the consequences deriving from the existence of a real space-time mechanism that enables atoms to communicate with one another. The term "communication" Bohr used entails once again the existence of two levels of interpretations:

1. There is a quantum radiation theory, which tackles the problem of the constitution of atoms and of radiation processes with a language suggested by acquired empirical knowledge, whose vocabulary is, however, extremely poor

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[37] Høffding was used to stress the concept of analogy in his *The Problems of Philosophy*. He stated that analogy is of great importance for description and discovery in science. In his letter to Høffding, dated September 22, 1922, Bohr wrote: «The relation you emphasize with regard to the role of analogy in scientific investigations is without doubt an essential feature of all natural sciences studies, although it does not always manifest itself clearly. It may often be possible to use a picture of a geometrical or an arithmetical kind that covers the problem dealt with, within the range under discussion, in so clear a manner that the considerations almost attain a purely logical character. However, in general, and especially in new fields of work, one must constantly bear in mind the apparent or the possible inadequacy of the picture and be satisfied as long as the analogy is so manifest that the utility, or rather the fruitfulness, of the picture, within the range where it is used, is beyond doubt. Such a relation holds not least at the present stage of the atomic theory. Here we are in the peculiar situation that we have fought our way to some information about the constitution of the atom, which must be considered just as certain as any of the facts of natural science. On the other hand, we meet difficulties of such a profound nature that we have no idea of the way to their solution; *my personal opinion is that these difficulties are of such a kind that they hardly allow us to hope, within the world of atoms, to implement a description in space and time of the kind corresponding to our usual sensory images*. Under these circumstances one must, of course, continually bear in mind *that one is employing analogies*, and the discretion with which *the areas of application of these analogies are defined in every single case is of decisive importance for making progress*». Italics of the present author. Bohr believed that the models of atomic structure have some realistic significance, but he appeared to be very cautious in employing classical models. From FAVRHOLDT (ed.), *NBCW*, 10 (cit. note 25, ch. 6), pp. 512-13.

and still only approximate (e.g. it includes the undefined terms "stationary state", "transition", and "communication mechanisms").

2. There is a classical theory, which possesses a vast store of knowledge expressed in a rigorous and highly formalized language but which cannot account for the world of micro-objects. The only thing to do, according to Bohr, is to use this second level systematically in order to gain as much information from it as possible and therefore endow quantum theory with new interpretative and conceptual tools[38].

Walther Bothe tried to test the Bohr-Kramers-Slater theory with the help of Hans Geiger, who had pioneered the methods of counting radioactive particles during his Manchester years with Rutherford. The Bothe-Geiger experiment soon yielded a result. On January 15, 1925 Max Born wrote to Niels Bohr: "The other day I was in Berlin. There everybody was talking about the result of the Bothe-Geiger experiment, which decided in favour of light quanta. Einstein was triumphant"[39].

They gave a report on the details of the experiment in a paper, entitled "Über das Wesen des Comptoneffekts; ein experimenteller Beitrag zur Theorie der Strahlung" (On the Nature of the Compton Effect; An Experimental Contribution to the Theory of Radiation) which was published on the *Zeitschrift für Physik* on April 25, 1925. In this paper Bothe and Geiger concluded:

«The experiments described here are not in agreement with Bohr's interpretation of the Compton effect […]. However, not only in respect to the Compton effect, but also in general, the result obtained above seems to present very great difficulties for Bohr's new radiation theory because the interpretation of the

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[38] Cf. PETRUCCIOLI, *Atoms, Metaphors* (cit. note 6, ch. 1), p. 121.

[39] Letter from Born to Bohr, January 15, 1925, in STOLZENBURG (ed.), *NBCW,* 5 (cit. note 20), 302-04.

Compton effect has been connected by Bohr, Kramers and Slater most closely with the statistical view of energy and momentum conservation, on which this new theory is based. One must reasonably well assumed that the concept of the light-quantum possesses a higher degree of reality than admitted in this theory»[40].

The results of the experiment were hence interpreted as a corroboration of the "photon" hypothesis. The reaffirmation of the validity of the conservation principles was seen as having crushed Bohr's program. However, Bohr's reaction was surprising. In a postscript to a letter to Ralph Fowler, dated April 21, 1925, he declared: "I was quite prepared to learn that our proposed point of view about the independence of the quantum process in separated atoms would turn out to be wrong. The whole matter was more an expression of an endeavour to attain the greatest possible applicability of the classical concepts than a completed theory"[41]. Bohr's massage was that the correspondence program merited the greatest respect because it had already positively achieved its purpose in spite of the experimental evidence as, in his view, the falsifiable content of the theory of the virtual oscillators involved only the classical model description, but not the idea to endow quantum theory with the conceptual tools of classical mechanics, i.e. the rational generalization thesis, which requires the condition of continuity. To conclude, Bohr tried in the context of the correspondence principle to "develop a description of optical phenomena in line with the quantum theory of

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[40] WALTHER BOTHE, HANS GEIGER, "Über das Wesen des Comptoneffekts; ein experimenteller Beitrag zur Theorie der Strahlung", *Zeitschrift für Physik*, 1925, 32: 639-63. From JAGDISH MEHRA, HELMUT RECHENBERG, *The Historical Development of Quantum Theory*, I (New York: Springer, 1982), pp. 611-12.

[41] Letter from Bohr to Geiger, April 21, 1925, in *NBCW*, 5 (cit. note 20), pp. 353-54.

spectra”[42] and it provided a clear signposts for the future construction of quantum theory.

10.4 The *Universal* Meaning of the Correspondence Principle

One of the most important aspects of Bohr’s philosophy is his claim that quantum mechanics is a “rational generalization” of classical mechanics. As he admitted in 1929:

«[…] Quantum mechanics may be regarded in every respect as a generalization of the classical physical theories»[43].

I think that the understanding of such a Bohr’s account may help to shed some light on the principle of correspondence, complementarity and the indispensability of classical concepts.

The centrality of the rational generalization thesis to Bohr’s philosophy is evidenced by the fact that it is a point that he makes repeatedly in his writings throughout his career. This thesis appears in the context of both the old quantum theory and the new (post 1925) quantum theory as it cut transversally the whole quantum mechanics. Bohr’s report to the third Solvay Congress in 1921 gave one of the first accounts with respect to the generalization thesis:

«It may be useful first to examine the general features of the theory more closely and especially to elucidate, on the one hand, the radical departure of the quantum theory from our ordinary ideas of mechanics and electrodynamics as well as, on the other hand, the formal analogy with these ideas. […]

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[42] “Addendum” of July 1925 added to the proofs of an article sent to the review *Zeitschrift für Physik* on March 30. Though the article on the behaviour of atoms during collisions contained a viewpoint which was by then obsolete in light of the new results, Bohr decided to publish it anyway; he added a note, however, in which, for the first time, he took an official stance on the meaning of the falsification of the virtual oscillators (“Über die Wirkung von Atomen bei Stössen” (On the Behaviour of Atoms in Collisions), *Zeitschrift für Physik*, 1925, 34: 142-57; text and English translation in STOLZENBURG (ed.), *NBCW*, 5 (cit. note 20), 194-206.

[43] BOHR, “Introductory Survey” (cit. note 1, ch. 1), p. 282.

The analogy is of such a type that in a certain respect we are entitled in the quantum theory to see an attempt of a natural generalization of the classical theory of electromagnetism»[44].

The last quotation clearly states that Bohr saw the development from classical electrodynamics to the old quantum theory as forming one continuous development. Bohr regarded the correspondence principle as the linchpin of this continuity. He also emphasized this point in 1929 when he wrote:

«We are dealing here with an unbroken development […] which, beginning with the fundamental works of Planck on black body radiation, has reached a temporary climax, in recent years, in the formulation of a symbolic quantum mechanics»[45].

Consequently, the last statements does uphold my claim that the correspondence principle was neither a formal analogy nor so radical a revision of the procedure that Bohr presented in the trilogy. On the one hand, Bohr himself admitted that he was dealing with an "analogy of such a type" that we are allowed to see in the quantum theory a "natural generalization" of classical theory. On the other hand, he spoke of "unbroken development" between the old and the new quantum mechanics. I daresay that we would not be obliged to embrace the 1918's formulation of the correspondence principle even if we were not inclined to recognize a conceptual leap between the first and the definitive version of such a principle. Furthermore, in 1925 Bohr went so far as to say:

«The whole apparatus of the [new] quantum mechanics can be regarded as a precise formulation of the tendencies embodied in the correspondence principle»[46].

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[44] ID., "On the Application" (cit. note 4), p. 366.

[45] ID., " The Quantum of Action and the Description of Nature" (cit. note 48, ch. 1), p. 92.

[46] ID., "Atomic Theory and Mechanics" (cit. note 45, ch. 2), p. 49.

This claim is puzzling in light of the widespread belief that Bohr's correspondence principle is just the requirement that, in the limit of large quantum numbers, there should be an agreement between the predictions of quantum and classical mechanics.

Thanks to the efforts of historians and philosophers of science such as Rud Nielsen[47], Batterman[48], Darrigol[49], Tanona[50], Bokulich P. and Bokulich A.[51] there was recognition that Bohr's correspondence principle was not seen as some sort of requirement that classical mechanics be recuperated from quantum mechanics in the limit of high quantum numbers. The first germ of the correspondence principle, as Bohr himself reported[52], can be found in his 1913's trilogy, although the term does not appear in his writings until 1920. It does mean that the correspondence principle is not only a technical tool, but is an essential part of quantum theory.

In calling quantum theory a generalization of classical mechanics, Bohr emphasized that there is a sense in which classical mechanics has not been replaced, but rather survives the quantum revolution in a new form. He explains:

«The problem with which physicists were confronted was therefore to develop a rational generalization of classical physics, which would permit the harmonious incorporation of the quantum of action»[53].

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[47] RUD NIELSEN, "Introduction to Part I", *NBCW,* 3 (cit. note 4), pp. 3-46.

[48] ROBERT BATTERMAN, "Chaos, Quantization and the Correspondence Principle", *Synthese*, 1991, 89: 189-227.

[49] OLIVIER DARRIGOL, *From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory* (Berkeley, CA: University of California Press, 1992).

[50] SCOTT TANONA, *From Correspondence to Complementarity: The Emergence of Bohr's Copenhagen Interpretation of Quantum Mechanic*s (Indiana University, PhD Dissertation, 2002).

[51] BOKULICH & BOKULICH, "Niels Bohr's Generalization" (cit. note 2, ch. 1).

[52] BOHR, "Seven Lectures on the Theory of Atomic Structure", 1922, in RUD NIELSEN (ed.) *Niels Bohr Collected Works*, Vol. 4: The Periodic System, 1920-1923 (Amsterdam: North Holland Publishing, 1977), 341-419, p. 348.

[53] ID., "Quantum Physics and Philosophy" (cit. note 59, ch. 3), p. 2.

It is worth remarking that Bohr used the correspondence principle for searching a way to generalize classical mechanics:

«The correspondence principle expresses the tendency to utilize in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the postulates and the classical theories»[54].

It is evident that Bohr's aim, here, is the reconciliation of classical and quantum theories by bringing them together into a rational and consistent whole, that it does not at all mean a merging of the two theories, but rather having found between them a connection of a purely qualitative nature. Bohr reiterated this view in 1939 while he emphasized the constant role of the correspondence argument from the old to the new quantum theory in generalizing the concepts of the classical physics:

«In the search for the formulation of such a generalization, our only guide has been the so-called correspondence argument, which gives expression for the exigency of upholding the use of classical concepts to the largest extent compatible with the quantum postulates»[55].

On this understanding of the correspondence principle, Bohr is not simply saying that the quantum theory should go over to the classical theory in the appropriate limit. Rather, he is maintaining that quantum mechanics should be a theory that departs as little as possible from classical mechanics. In this sense we can affirm that Bohr's emphasis on continuity was not just a heuristic for theory construction, but was also an essential condition of what he took to be the proper understanding and interpretation of quantum theory.

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[54] ID., "Atomic Theory and Mechanics" (cit. note 45, ch. 2), p. 37.

[55] ID., "The Causality Problem in Atomic Physics", *New Theories in Physics* (Paris: International Institute of Intellectual Co-operation, 1939), 11-30, p. 13. Reprinted in KALCKAR (ed.) *Niels Bohr Collected Works*, Vol. 7: Foundations of Quantum Physics II, 1933-1958 (Amsterdam: North-Holland, 1996), 303-322, p. 305.

10.5 Continuity as a Condition for Objective Knowledge

Since 1928 Bohr spoke constantly of the "failure of the forms of perception adapted to our ordinary sense impressions"[56]. Bohr included causality as well as space and time in the forms of perception:

«Causality may be considered as a mode of perception by which we reduce our sense impressions to order»[57].

Bohr's inappropriate use of "forms of perception" with respect to causality derives from Høffding, who talked of the "forms of understanding". As is known, according to Kant, causality is a category, a concept of understanding, rather than a form. As I showed on chapter 5, paragraph 5.1, Høffding regarded continuity as an essential condition that his theory of knowledge requires to the forms of perception and the categories of cognition. Following Høffding, Bohr thought of space, time, causality as forms of perception, and made no mention of Kant, probably because he had no first-hand acquaintance with his writings. The forms of perception are thus the means by which we organize our perceptual experience. They express the conceptual structure of classical physics, through which macroscopic world can be interpreted. However, the classical forms of perception are not necessarily applicable to microphysical world. According to Bohr, the breakdown of visualizability in microphysics is due not just to the peculiar feature of micro objects, but to the failure of continuity in Høffding's sense. Coherently, in 1926-27 Bohr stated: "the definition of every word essentially presupposes the continuity of phenomena and becomes ambiguous as soon as this presupposition no

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[56] ID., "The Quantum of Action" (cit. note 48, ch. 1), p. 93.

[57] ID., "Atomic Theory and the Fundamental Principles Underlying the Description of Nature", in *Atomic Theory* (cit. note 1, ch. 1), 102-119, pp. 116-17. Reprinted in *The Philosophical Writings*, 1 (cit. note 45, ch. 2).

longer applies”[58]. On this view, ordinary language is designed for organizing our sensory experience and it presupposes the condition of continuity. As D. Murdoch noted, if this presupposition failed, ordinary language, including the models in terms of which we interpret physical theory, would become ambiguous[59].

[58] Letter from Bohr to Schrödinger of December 2, 1926, in *NBCW*, 6 (cit note 1, ch. 1), p. 462.

[59] DUGALD MURDOCH, *Niels Bohr’s Philosophy of Physics* (Cambridge, GB: Cambridge University Press, 1987), p. 73.

Chapter 11

Conclusion

11.1 The Physical Implications of Unity and Continuity as a Heuristic

After 1913 one of the main problems in the interpretation of quantum theory, according to Bohr, was the reconciliation of the idea of the discontinuous exchange of energy between the atom and the radiation field with the classical notion of the continuous transmission of radiation *in vacuo*. Bohr came to adopt a viewpoint for which the light-quantum hypothesis and the wave hypothesis mutually exclude each other. In fact, he was not inclined to accept the light-quantum hypothesis, for he believed that only the wave hypothesis could provide a satisfactory explanation of interference phenomena. In 1920 Bohr states that such phenomena "cannot possibly be understood on the basis of a theory such as that of Newton […] In fact, the picture provided by Einstein's conceptions looks very much like Newton's, and it can no more than that give any sort of explanation of the interference phenomena"[1].

In Bohr's view, the light-quantum model simply illustrated the difficulty of reconciling the discontinuity inherent in atomic processes with the continuity presupposed by classical electrodynamics.

«The hypothesis of light-quanta, therefore, is not suitable for giving a picture of the processes, in which the whole of the phenomena can be arranged […] The satisfactory manner in which the hypothesis reproduces certain aspects of the phenomena is rather suited for supporting the view […] that, in contrast

[1] BOHR, "Om Vekselvirkning mellem Lys og stof" (On the Interaction between Light and Matter), in ROSENFELD, RUD NIELSEN (eds.), *NBCW*, 3 (cit. note 4, ch. 10), 228-240, pp. 229, 234.

to the description of natural phenomena in classical physics in which it is always a question only of statistical results of a great number of individual processes, a description of atomic processes in terms of space and time cannot be carried through in a manner free from contradiction by the use of conception borrowed from classical electrodynamics […]»[2].

It was noted that Lorentz's paper of 1910 on the light-quantum had a profound influence on Bohr. Oskar Klein recalled that Bohr frequently mentioned Lorentz's arguments for the incompatibility between the light-quantum hypothesis and the fact concerning the interference of light waves[3].

Nevertheless, until 1925 Bohr was preoccupied nor by the wave-particle duality neither the continuity-discontinuity duality. The main problem he had hitherto faced was the apparent inconsistency between the quantum theory, which implies discontinuous emission or absorption of radiation, and the electromagnetic theory, which implies continuous emission and absorption. This also led to what I have termed the gnoseological issue of continuity. In order to reconcile the discontinuity inherent in the quantum theory and the continuity of the classical theory, what was required, for Bohr, was a radical departure from classical mechanics.

Moreover, it was in the attempt to reconcile the continuity implicit in the classical theory with the discontinuity presupposed by the quantum theory that the Bohr-Kramers-Slater theory of virtual oscillators regarded the atom in a stationary state as a set of virtual harmonic oscillators, whose frequencies are those of possible transitions

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[2] ID., "On the Application of the Quantum Theory to Atomic Structure", *Proceedings of the Cambridge Philosophical Society* (Supplement) (1924), 1-42. Reprinted in *NBCW*, 3 (cit. note 4, ch. 10), 457-99, p. 492.

[3] Interview with Oskar Klein, *Archive for the History of Quantum Physics* [AHQP], February 15, 1963, Session 6, p. 1.

between stationary states, and that atoms communicate with one another by means of virtual radiation fields produced by the oscillators[4].

Some scholars noted that it was around Bohr's concepts of stationary states and quantum jumps, rather than the wave-particle dilemma or indeterminism, that the new philosophy of physics was erected[5]. As is known, the idea of stationary states, and especially the idea of instantaneous transitions between such states, implied from the very beginning of Bohr's program a radical departure from classical space-time models. But the wave theoretical model would allow Bohr finally to decipher the limits of the application of space-time to the atomic model, and above all, the wave model would enable him to resurrect his idea of the stationary states of an atom.

Bohr's program was hence characterized by the attempt to reconcile conceptually quantum theory and classical theory, and this resurfaced also in the central message of Bohr's Come lecture: the compatibility, indeed, the "happy marriage", between Schrödinger's successful continuous wave mechanics and the quantum postulate. In fact, the Como lecture announced a resolution of these long-standing problems (atomic structure, the interaction of radiation with matter, and collisions) by harnessing the wave theoretical framework to the quantum postulate. This is to say that Lakatos was right in defining the stationary states and the quantum jumps as the starting point, or the irrefutable nucleus of Bohr's research program; indeed the concept of the stationary states was beset with conceptual inconsistencies from the start. Moreover, as it was shown, the correspondence rule was a heuristic to finding a satisfactory quantum theory. As is known Lakatos subscribed this point, even if he took no heed of the concept of

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[4] Cf. MURDOCH, *Niels Bohr's Philosophy* (cit. note 59, ch. 10), pp. 23-24.

[5] Cf. MARA BELLER, *Quantum Dialogue. The Making of a Revolution* (Chicago, IL: The University of Chicago Press, 1999), pp. 118-44.

continuity as an essential condition for the correspondence principle. This un-verifiable and un-falsifiable statement explains why, according to Bohr, quantum mechanics was a rational generalization of classical mechanics, even though the quantum postulate and the quantum jumps were taken into account.

Bohr acknowledged, indeed, that the correspondence argument failed in those cases where particular non-classical concepts have to be introduced into the description of atoms. But he still thought that the correspondence argument was indispensable for both structural and semantic reasons in constructing a proper quantum theory as a generalised theory from classical mechanics.

The continuity inherent in the correspondence principle was essential to Bohr's theory of knowledge as it was a precondition for the acquisition of empirical knowledge. In fact, as is known, the quantum of action created a problem for the possibility of causal description of individual atomic objects, required by the ideal of causation, inasmuch as the causal mode of description had to be considered complementary to the spatio-temporal mode of description. Bohr grasped the difficulty of applying the causal mode of description to the domain of quantum mechanics, where the subject-object distinction is blurred due to the character of wholeness and unity introduced by the quantum of action. Just like Høffding had seen the difficulties in psychology and biology as consequences of the application of causal and holistic descriptions respectively. Bohr perceived that the solutions to such difficulties lie in the use of complementary descriptions in quantum mechanics, as well as in psychology and biology.

It is generally assumed that the roots of Bohr's complementarity lie in the experimental refutation of the Bohr-Kramers-Slater theory. As Mara Beller noted, this theory used the wave theoretical framework of light exclusively rather than Einstein's theory of light quanta. After the Bothe-Geiger experiments – many scholars argued – Bohr wouldn't have had no choice but to assimilate light-quanta, which he had rejected vigorously beforehand. However, Bohr did not adopt the idea of point-like light-quanta even after the Bothe-Geiger experiments. Bohr's complementarity principle implied further rejection, not acceptance, of the idea of point-like material particles.

It is worth remarking once again the conceptual symmetry between the correspondence principle and the use Bohr made of the wave concepts. Bohr's argument for the superiority of wave concepts does not mean that waves represented a literal picture of reality, but rather the wave model was heuristically more useful. Mara Beller sustained that the fruitfulness of the concept of wave packets is especially evident in the case of interactions. We will never observe either an isolated particle or a monochromatic wave, but only cases of superposition of light or matter waves. Last but no the least, the idea of a wave packet is particularly suitable for demonstrating the harmony between the possibilities of wave theoretical definition and those of observation[6].

I would add in addition to Bohr's account that the concept of continuity correlated to the idea of wave packet represents the condition to get that asymptotic unity or harmony between observation and definition of concepts, i.e. reality and theory, which I have referred to on chapter 3. The whole Bohr's program appears to be informed by such interaction between theory and reality. Returning to the heuristic value of the idea of wave packets, characterized by the condition of continuity, we can now show how step-by-step the physical fruitfulness of this idea rose.

[6] Cf. BOHR, "The Quantum Postulate and the Recent Development of Atomic Theory" (Unpublished manuscripts, October, 12-13, 1927) in KALCKAR (ed), *NBCW*, 6 (cit. note 1, ch. 1), 89-98, p. 97.

We now need to heed the idea of stationary states and the quantum jumps, i.e. the starting point of Bohr's program. We have to remind that the whole Bohr's program was an attempt to make such idea compatible with the new physics in order to re-establish the condition of continuity. Indeed, the stationary states imply the idea of discontinuity. As it was rightly said, Bohr's aim was not to save a program, rather to carry through its hard core in spite of the internal inconsistencies by means of a departure by classical models. Even in the Bohr-Kramers-Slater theory the idea of stationary states and quantum jumps necessitated such major departures as non conservation of energy and momentum, as well as the introduction of the strange "virtual field" that an atom in a definite stationary state emitted.

Moreover, the Bohr-Kramers-Slater theory implied further departures from ordinary space-time visualization, for example, in the explanation of the Compton effect. Similarly, the analysis of the phenomenon of collisions, when a fast-moving particle collides with an atom in a certain stationary state, demanded resignation from strict conservation of energy and momentum[7]. When Bohr learned of the results of the Bothe-Geiger experiments, confirming strict conservation, he did not question the formulas of the wave theory of light or the adequacy of the description of atoms in terms of stationary states. It was rather regarded by Bohr as a further departure from classical space-time models for the interior of the atom. Bohr was supported in this direction by the wave theoretical ideas of de Broglie and Einstein. The paper Bohr wrote on these matters was published in 1925, just before Heisenberg's *reinterpretation* paper ("Quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen", *Zeitschrift für Physik*, 1925, 33: 879-93, in English "Quantum-theoretical

[7] Cf. ID., "Über die Wirkung von Atomen bei Stössen" (cit. note 42, ch. 10).

Reinterpretation of Kinematical and Mechanical Relations"). Heisenberg's solution, however, was too radical. Matrix mechanics could not theoretically describe the states of atomic systems and the evolution of phenomena. Schrödinger indeed perceived the matrix mechanical formalism as eliminating Bohr's concept of separate stationary states. Schrödinger himself attempted a continuous wave description that would further eliminate the concept of stationary states with definite energies, and discontinuous transitions between these states, by suggesting instead a resonance model in terms of frequencies. As Mara Beller has argued, the Göttingen and Copenhagen physicists joined forces in response to the perceived threat from Schrödinger – his attempt to reduce Bohr's concepts to the status of Ptolemaic epicycles.

It was around the adequacy of Bohr's concepts of stationary states and quantum jumps that the crucial interpretative attempts resolved[8]. Bohr, who accepted the great usefulness of Schrödinger's formalism, could not see initially how solutions to Schrödinger's wave equation, in terms of the superposition of different proper vibrations, could be reconciled with the idea of separate stationary states. After Schrödinger's visit in Copenhagen in 1926, the direction of Bohr's efforts became clear: to achieve compatibility between wave theoretical ideas and the quantum postulate. As Heisenberg put it:

«Bohr realized at once that it was here we would find the solution to those fundamental problems with which he had struggled incessantly since 1913 and in the light of the newly won knowledge he concentrated all his thought on a critical test of those arguments which had led him to ideas such as stationary states and quantum transitions»[9].

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[8] BELLER, *Quantum Dialogue* (cit. note 5), p. 126.

[9] HEISENBERG, "Quantum Theory and Its Interpretation" (cit. note 2, ch. 4), p. 101.

Bohr's Como lecture was the culmination and resolution of these efforts. Bohr's criticized Schrödinger's attempts to replace "the discontinuous exchange of energy […] by simple resonance phenomena"[10]. Schrödinger's theory must necessarily be interpreted by an explicit use of the quantum postulate, and in direct connection with the correspondence principle.

Furthermore, if according to Schrödinger's equation, an atom exists in a superposition of proper vibrations, consequently it appeared difficult to reconcile the idea of stationary states with Schrödinger's wave function. In fact, according to the quantum postulate, an atom should be always in some definite stationary state. Bohr's ingenious resolution of this difficulty was guided by the search for the compatibility of the possibilities of *definition* and *observation*: the superposition of proper vibration stands for a *definition* (that is the "theory"; as it is expression of the continuity inherent in Schrödinger's equation) of the interactions (observations)[11]. But an atom in a definite stationary state with precise energy is not accessible to observation, therefore it can correctly be described as a single proper vibration. It means that the idea of a stationary state becomes an abstraction, both because it is represented by a single wave and because a system not accessible to observation constitutes in a certain sense an abstraction, just as an idea of an isolated particle. However, the concept of stationary state was essential in Bohr's view for the interpretation of phenomena in spite of its abstract meaning because "the conception of a stationary state involves, strictly

[10] BOHR, "The Quantum Postulate and the Recent Development of Atomic Theory", *Nature*, 1928, 121 (suppl.): 580-90. Reprinted in *NBCW*, 6 (cit. note 1, ch. 1), 148-58, p. 154.

[11] BELLER, *Quantum Dialogue* (cit. note 5), p. 127.

speaking, the exclusion of all interactions"[12], the constant energy value associated with such states "may be considered as an immediate expression for the claim of causality contained in the theorem of conservation of energy"[13].

Beller reminds that Bohr devoted all of paragraph 5 of the Como lecture to arguing for the consistency of the concept of a stationary state. Bohr's strategy was to demonstrate that complementarity between stationary states and corpuscular space-time descriptions – or between causality and space-time, for the interior of an atom – accords fully with the possibilities of observation. Nevertheless, the concept of complementarity between stationary states and corpuscular space-time descriptions presented a serious problem in the case of large quantum numbers, where, according to Bohr's correspondence principle, the concept of stationary states would approach the classical space-time orbits along which intra-atomic particles revolve[14]. Schrödinger arose a legitimate objection: how to regard the concept of stationary states in the case of an electron moving along an orbit with a large quantum number, which must be represented by a wave packet, i.e. a superposition of many vibrations? This is to say: how can complementarity, or the mutual exclusion of the concept of stationary states and individual particles, possibly be maintained in the case of large quantum numbers?

Once again it is worth noting that the harmony between the possibilities of observation and definition, that is, between reality and theory, played a guiding role in searching for a solution.

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[12] BOHR, "The Quantum Postulate" (cit. note 10), p. 155.

[13] *Ibidem*.

[14] Cf. BELLER, *Quantum Dialogue* (cit. note 5), p. 128.

When we identify the exact value of the energy of a stationary state by means of collisions or radiation reaction, we inevitably imply “a gap in the time description, which is at least of the order of magnitude of the periods associated with transitions between stationary states. In the limit of high quantum numbers these periods, however, may be interpreted as periods of revolution”[15].

The possibility of describing stationary states by means of Schrödinger’s wave function indicated to Bohr the physical significance of wave mechanics and the possibility of resurrecting classical mechanics. The fact that such a definition of a stationary state is theoretically adequate was demonstrated, according to Bohr, by Born’s work, which provided “a complete description of the collision phenomena of Frank and Hertz, which may be said to exhibit the stability of stationary states”[16]. On purpose Bohr stated:

«Notwithstanding its remarkable adaptations to the requirements of quantum theory, Schrödinger’s wave equation theory will appear to constitute a transcription of classical mechanics which for the time being can be interpreted only by the explicit use of the quantum postulate and the concepts of material particles. This had been emphasized especially by Born, who in connection with his important investigation of collision problems has suggested a simple statistical interpretation of Schrödinger’s wave functions»[17].

Furthermore, the year before, Bohr had written to Ralph Fowler: “just in the wave mechanics we possesses now the means of picturing a single stationary state which suits all purposes consistent with the postulates of the quantum theory. In fact, this is the very

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15 BOHR, “The Quantum Postulate and the Recent Development of Atomic Theory” in *Atti del Congresso dei Fisici Italiani*, September, 11-20, 1927 (Bologna: Zanichelli, 1927), 565-588. Reprinted in *NBCW*, 6 (cit. note 1, ch. 1), 113-136, p. 135.

16 ID., “Philosophical Foundations of the Quantum Theory” (Unpublished manuscripts, 1927) in *NBCW*, 6 (cit. note 1, ch. 1), 67-72, p. 71.

17 ID., “The Quantum Postulate” (cit. note 6), p. 97.

reason for the advantage which the wave-mechanics in certain respects exhibits when compared with the matrix method"[18].

Schrödinger's version of quantum mechanics, in Bohr's view, introduced a model that resurrected both the stationary states and the idea of quantization. It also allowed the physical interpretation of quantization, i.e. the visualization, which was possible thanks to the condition of continuity re-established by the wave-theoretical model along the line of the classical physics:

«[…] Schrödinger has expressed the hope that the development of the wave theory will eventually remove the irrational element expressed by the quantum postulate and open the way for a complete description of atomic phenomena along the line of the classical theories»[19].

Moreover, the crucial point was that matrix mechanics defied any physical interpretation, while Schrödinger's wave mechanics allowed an understanding of the electric and magnetic properties of the atoms:

«Furthermore, the number of nodes in the various characteristic vibrations gives a simple interpretation to the concept of quantum number which was already known from the older methods but at first did not seem to appear in the matrix formulation»[20].

Nevertheless, the main difficulty arises in the case of intra-atomic particles, which cannot be visualized in terms of ordinary space-time picture even though they are represented by a wave theoretical framework. As Bohr himself noted, it is here that the most striking difference between classical and quantum mechanics lies: while in the former "particles are endowed with an immediate 'reality', independently of their being

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[18] Bohr to Fowler, October 26, 1926, *AHQP*.

[19] BOHR, "The Quantum Postulate" (cit. note 10), p. 154.

[20] *Ibidem*.

free or bound", in quantum mechanics intra-atomic, bound particles are not visualizable[21]. For this reason Bohr found both Schrödinger and Heisenberg's approaches inadequate. Bohr agreed with Heisenberg that Schrödinger's waves in multidimensional space could hardly be considered immediately intuitive. But neither was Heisenberg's approach satisfactory: the intra-atomic reality was not a particle-kinematic one. Moreover, Schrödinger saw Bohr's argument in the Como lecture as a defence of the concept of stationary states. Then Schrödinger argued that the combination of uncertainty relations and the idea of stationary states destroys compatibility between definition and observation. The controversy was not resolved and Schrödinger still in the 1950s continued to criticize Bohr's idea of stationary states and quantum jumps.

Returning to Lakatos' evaluation of Bohr's research program, we may say that he correctly put emphasis on the fundamental aspect of Bohr's atomic theory: the hard core, i.e. the starting point of his program, and he regarded such a program as progressive at least until 1925. However Lakatos failed to understand the essential requirement of continuity underlying Bohr's program. It was for the condition of continuity that Bohr himself did not consider the transition from classical to quantum physics as a revolution. In fact, Bohr's methodological prescriptions in terms of correspondence rule were very different, for instance, from a Kuhnian prospective. Where Kuhn saw revolutions and incommensurability, Bohr looked for rational generalizations and commensurability. Furthermore, what Lakatos did not accept as regards Bohr's program was the concept of complementarity that represented for him a kind of religion or philosophy introduced in order to save the program. According to

[21] *Ibidem*, p. 155.

Lakatos, after 1925 Bohr was responsible for an unprecedented lowering of critical standards for scientific theories. Einstein defined the Heisenberg-Bohr new quantum mechanics as a sort of tranquillizing philosophy, or religion, which provided "a gentle pillow for true believer"[22]. In my view, as I have shown, complementarity was the culmination of Bohr's research program. For this reason Bohr could be characterized more as an evolutionary theorist than a revolutionary one, as I wrote in the introduction to this dissertation. Following Michael Friedman and Jan Faye, I will claim that Planck's discovery of the discontinuity of the black body spectrum was considered by Bohr as a well-established empirical fact, which was elevated to a constitutive (a priori) principle in his model of the atom. This semi-classical model was applied to the hydrogen atom. From then on, a continuous struggle began of enlarging the model to deal with atoms with higher numbers and to reach a proper theory, which could explain the dynamics of elementary particles. One may therefore argue that the period between 1913 and 1925 was one long process of conceptual adaptations, which involved a collection of the most brilliant physicists at that time. This process only partly ended in 1925 when Heisenberg established a proper theory of quantum mechanics in which the quantum of action still formed the constitutive a priori principle. However, it is worth remarking that still in 1927 Bohr saw in Schrödinger's version of quantum mechanics a way of removing the irrational element introduced by the quantum of action by developing the wave theory. In this sense, the thematic concept of unity or harmony between description and observation, and the condition of continuity, i.e. the essential aspect for communicating any kind of

[22] Letter from Einstein to Schrödinger, May 31, 1928, in KARL PRZIBRAM (ed.), *Briefe Zur Wellenmechanik* (Vienna: Springer, 1963).

concepts by means of the classical theory (the ordinary language), would represent the real philosophy of Niels Bohr.

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The present work has been rendered possible thanks to the use of primary sources from:

- The Niels Bohr Archive: in particular, Bohr Private Correspondence, Bohr Scientific Correspondence, Bohr Scientific Manuscripts.
- The Archive for the History of Quantum Physics (AHQP).
- The American Institute of Physics.

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